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MEDICAL AND PHYSIOLOGICAL

PROBLEMS,

BEING CHIEFLY RESEARCHES

FOR

CORRECT PRINCIPLES OF TREATMENT

IN DISPUTED POINTS OF

MEDICAL PRACTICE.

BY

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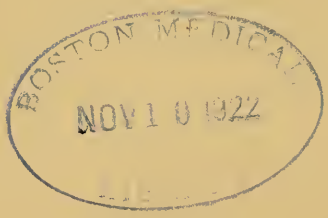

PREFACE.

THERE are two *modes* of treating all diseases, a right and a wrong one; and it is popularly believed that the science of Medicine has long since determined between them; that in every dangerous case, or, at all events, in those of ordinary or frequent occurrence, the practitioner has only to refer to received principles or authorities on the subject, and that if he commits an error in his selection of remedies, it is entirely attributable to his want of information or ability. It would, no doubt, astonish the public very much, and do little credit to medical art, if they were told, that there is scarcely a complaint to which humanity is liable, the treatment of which is, in all points, absolutely agreed upon by the profession; and that with reference to the most ordinary of dangerous cases, and those supposed to be best understood, remedies of a different and even opposite nature have been advocated by men of the highest celebrity. It is little imagined the deep responsibility that rests with the young physician, when a decision is demanded of him on such occasions, or the profound judgment, as well as extensive information required to arrive at a correct conclusion between conflicting authorities of equal consideration. Whatever his decision may be, if he is himself sufficiently informed of the difficulties, he must act under the depressing assurance, that if the result is fortunate the judgment evinced in overcoming them will never be appreciated, while, if it be otherwise, there will not be wanting persons to inquire why other modes of treatment were not adopted. It occurred to me

some years since, that it would be a most useful, practical and interesting study to collect causes with reference to all the most important of the disputed points in practice, and afterwards as a sufficient degree of personal experience happened to be attained on any one of them, to review the opinions of all the best authors on the subject, and endeavour to solve that most difficult question —What is the correct principle of treatment? The following papers which my brother and I have contributed from time to time to the medical periodicals, are the results of these reflections. They are now collected for the convenience of easier reference, when it is an object to ascertain the value of opposing doctrines on the treatment of diseases yet unsettled. Some physiological problems have also been discussed, the interest of which may atone for their want of any very obvious practical value.

The short essay on the application of mathematics to the science of medicine with which the collection concludes, could no where be perused with so much interest, as after the mind has been perplexed by the surpassing difficulties of medical doctrines, displayed in the preceding pages. Amidst all the uncertainty of the subject, it is consoling to observe that a method is at length attracting the attention of every philosophic enquirer, which promises one day to give medicine a place among the more certain sciences.

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MEDICAL AND PHYSIOLOGICAL PROBLEMS.

PROBLEM I.

BY WILLIAM GRIFFIN, M. D.

WHAT PRINCIPLES SHOULD BE KEPT IN VIEW BY THE PHYSICIAN IN THE TREATMENT OF ENTERITIS.

After the great accessions which have been made to our knowledge of the pathology of the alimentary canal, within the last few years; it may appear extraordinary that I should propose the foregoing as a question difficult of solution. In some points in the treatment of intestinal inflammation, there is, I am aware, a very general agreement among medical men; but in others, and by far the most important, I am far from imagining the practice is at all settled or uniform; there is, indeed, among persons of considerable reputation, not only a difference, but a direct opposition of opinion, which sometimes occasions a difficulty in the management of such cases, only conceivable in its full extent by those who have had to stand at the bed side, and choose between the two, with the conviction that life or death hung upon the decision. But even if the practice was more uniform, until some general principles of treatment are universally admitted, there will always be ground for perplexity in the timing of remedies, in determining the changes which are to give us our indications, and in fixing the moment when it will be safe to commence or abandon a particular plan of management. Perhaps much of the discrepancy of opinion that exists might be explained by the success or ill success of remedies under the very varying conditions of the disease in which they have been administered. At all

events it would be rendering science some service, if even an approach to truth could be attained, and this is the utmost I would hope for, until the records of such cases as I shall here offer to the reader's attention, and of those treated on an opposite plan are multiplied and compared.

One might suppose, that at least with respect to the most important of our remedies in all inflammations—blood-letting, there must be a perfect agreement among medical men as to its application in enteritis. Perhaps this agreement may be said to exist within the first twenty-four or thirty-six hours from the commencement of the complaint, but certainly not afterwards.—Dr. Parr—no light authority even at the present day—says, "In the treatment of enteritis there is much doubt. We are ordered to bleed freely, though the pulse is small, and to repeat it till the pulse rises. When the patient is a robust countryman, and the disease induced by drinking cold fluids in a heated state, this advice may be useful, but it is not generally so. Perhaps bleeding is more seldom necessary in this disease than in any other inflammation; for it rapidly tends to mortification, and should it not at once relieve, it soon proves fatal."

Dr. Mason Good, whose work may be regarded as a compendium of the practice of some of the first physicians in this and other countries, and whose learning and ability must necessarily attach considerable weight to any opinion he supports, speaks of bleeding in much the same qualified manner with Dr. Parr, and says, if it does not succeed, it will assuredly hasten the stage of gangrene, and abbreviate the term of remediable operations. And again, he says, unless the constitution is tolerably vigorous, and the lancet is employed early, there is no inflammation in which the latter is less likely to be serviceable, or may become more mischievous. Dr. Abercrombie has none of these qualifications: he of course admits, as every one must, that bleeding in inflammation of any vital organ, to be of much service, should be used at an early period; but he does not seem to apprehend that it can be readily over done in the first instance, or that its repetition in the advance of the disease is useless, much less mischievous. He recommends after a first full bleeding, smaller ones every hour or two, until the force of the attack is broken; and seems to consider, that the repetition must at any period be directed by the degree of inflammatory action and strength of the patient's con-

stitution, without reference to its supposed effect in accelerating the approach of gangrene. Dr. Elliotson says, "the first thing one has to do is to bleed the patient well; you should set him as upright as he can be, and bleed from a large orifice without any mercy. You should of course consider the patient's strength; but you should bleed on till you make a decided impression—till you knock down the pulse, and make him faint." These are discrepancies among respectable authorities, sufficiently alarming at the very outset, but they fall far short of others, which the inexperienced physician will find himself surrounded by, at his next step in the treatment—the administration or rejection of purgatives.

To revert to Dr. Parr again, he says: "The salutary termination of the disease is by a discharge of fæces; if this can be obtained the patient is safe." Should mild purgatives prove ineffectual, he recommends that the more acrid ones be resorted to, and gives the same advice respecting injections. Dr. Pemberton, after premising general and local blood-letting, recommends, "if the stomach will bear liquids of any sort, a strong solution of epsom salts in mint water, with an addition of tincture of senna in such quantities, and at such intervals, as the sickness of stomach will allow. If, however, all liquids are rejected, we may direct an usual dose of calomel in union with the compound extract of colocynth every six hours, *ad quartam vicem.* In the intermediate hours, an injection of water-gruel with common salt may be employed. Purgatives are to be continued during the whole course of the complaint." Dr. Mason Good says, "from the first we must attempt cathartics, and if the stomach will not retain the milder, we must have recourse to the more acrid." He asserts, "it does not necessarily follow, that the irritation of those more acrid purgatives will add to the imflammatory irritation," and "that the cure depends almost entirely on our success in procuring free evacuations." Dr. Elliotson says, "after bleeding, undoubtedly a very large dose of calomel should be exhibited; after it has been taken some time, other purgatives should be given with active injections, so that if possible they may meet half way and combine, and then," as he expressively asserts, "*out goes the disease.*" "We should first," he tells us, "bleed freely, because purgatives will not operate until we have done that; we should then give a large dose of calomel, such as a scruple, by

the mouth, and a strong purgative injection, with plenty of salts, or salts and senna, or colocynth, or oil of turpentine, and repeat the calomel in ten grain doses every four or six hours, *giving purgatives in addition from time to time.*"

Let us contrast these with other opinions of the very highest character. Dr. Abercrombie says, "we have seen the bowels obstinately obstructed, and we have seen them spontaneously open or easily regulated, and in both cases the disease has run its course with equal rapidity to a fatal termination. We have found no reason to believe that the retention of fæces was in itself injurious in one case, or the free evacuation of them beneficial in the other; on the contrary, we have had evident reason to believe *that in several cases in which the inflammation appeared to be subdued, the action of a purgative was immediately followed by a renewal of the symptoms.* Along with these considerations, we must keep in mind the fact, that in the ordinary cases of enteritis, the action of purgatives is in general entirely fruitless; they are usually vomitted as often as they are given, and consequently can only prove additional sources of irritation. I know that much difference of opinion exists upon this subject among practical men; but upon the grounds now referred to, *I confess my own impression distinctly to be, that the use of purgatives makes no part of the early treatment of enteritis; on the contrary, they are rather likely to be hurtful until the inflammation has been subdued.* When we have reason to believe this has taken place, the mildest medicines or injections will often be found to have the effect, after the most active purgatives had previously been given in vain. In the general treatment of enteritis indeed it is desirable to keep the bowels if possible free from distention; but this object may, I think, be usually obtained by mild injections, or by the tobacco injection." Dr. Gregory, of London, and other physicians of equal respectability, entertain similar views with Dr. Abercrombie in the treatment of this disease. Broussais limits the use of purgatives to the close of the acute stage, and Armstrong, of whose practice I shall have to speak again presently, leaves them altogether out of sight, or mentions them only as requisite when the inflammation is over. It remains for us amidst these conflicting opinions to inquire anxiously on which we are to rely in practice. The following interesting case may assist us in determining the question.

Case I.—A lady, aged thirty-two, while engaged with an evening party, on the 1st of November, was affected with pain in the lower part of the back, and great weariness. She took three glasses of port wine with the hope of relieving it, but they did her no service; she became feverish in the night, and in the morning (2d) had headache and thirst with a pulse at 90; she was ordered some castor oil. At three in the afternoon she complained of severe pain in the stomach, and on examination the whole of the abdomen was found excessively tender to the touch, and somewhat full; she was warm and restless, and had an anxious and painful expression about the brow. *The castor oil had operated two or three times freely.* As I could not detect any tenderness of the lumbar spine, although she still felt the pain about the sacrum, I was satisfied the attack was purely inflammatory, and placing her upright in the bed, I opened a vein in the arm. Eight ounces of blood were hardly drawn when she fainted; the orifice was closed; she got some warm drink, and was laid in the horizontal position until she had perfectly recovered. The orifice was then re-opened, keeping her in the recumbent posture, but after losing two ounces more, she fainted again. Finding it impossible to procure the desirable quantity of blood in this way, I applied two dozen leeches, chiefly to the right hypochondriac and iliac region, which were more exquisitely tender than other parts of the abdomen, and gave three grains of calomel and a grain and a half of opium, repeating two grains of the former, and one of the latter, every second hour. The leeches drew about eight ounces of blood, and a great deal drained away through the remainder of the night after they fell off. In the morning (the 3d) the pain still continued extremely severe, and the tenderness was so acute, that she could not turn to either side, and scarcely could bear the bed-clothes to rest on her; it extended over the whole of the abdomen. I now took twenty ounces of blood from the arm freely, and without producing faintness. As the bowels had not been moved since they were affected by the castor oil in the middle of the day before, and there was some tumidity of the abdomen, pills composed of equal parts of aloes and extract of henbane, were ordered, and an oatmeal tea injection with castor oil, and two teaspoonfuls of spirits of turpentine. On administering this last she was seized with a dreadful forcing or bearing down pain in

the rectum, and passed nothing; the pain seemed as excruciating as any that could occur in violent labour, lasted for about twenty minutes, and was then relieved by the warm bath. In two hours afterwards the administration of a simple injection of oatmeal tea was followed by similar suffering, and was in like manner retained. The permanent pain was at this period severest in the left iliac region and about the navel, where the tenderness on pressure was extreme; the countenance was more anxious; the tumidity of the abdomen was increasing, and the stomach beginning to reject the drink. In consultation with another physician, it was now agreed to take blood again, and eighteen ounces more were drawn, being the third general bleeding within twenty-four hours. Two grains of opium and a grain of calomel were given immediately after, and ordered to be repeated every two hours through the night. In the morning (the 4th) there was a considerable improvement; the abdominal tenderness was diminished, the pain and sickness of the stomach had very much subsided, and the injections had come away with some dark, thin, feculent matter; she still, however, felt pain and a sense of great weariness at the lower end of the sacrum, shooting up through her back; and she had a great difficulty in passing water. She now informed me, that a few days previous to her present illness, she was attacked with a profuse leucorrhœal discharge, attended by heat and sense of scalding, but that it had since abated or almost ceased. A fomentation to the lower part of the abdomen was ordered, and the opium was continued in two grain doses, every two hours, without calomel. In the evening the improvement appeared progressive; the skin was cool; the pulse soft at one hundred and ten; the tongue cleaner; the abdomen was still full, but it had nearly lost its tenderness, and she could turn in the bed with little pain. She spent the night and the following day (the 5th) with little uneasiness except for the soreness in the lower bart of the back, and difficulty in passing water, which sometimes occasioned violent straining and bearing down.— Though suffering much in this way, she would not permit the removal of the water by the catheter. She now for the first time asserted, that matter was passing from the rectum; it was, however, supposed to be merely a return of the leucorrhœal discharge; no further attention was paid to it; and as there were so many other symptoms of amendment, the opium was ordered

to be given in grain doses only, every fourth hour. She had already taken thirty-two grains within the last thirty-two hours, without its occasioning stupor, headache, or any other unpleasant symptom, except on the last night, when she complained of frightful dreams and startings. On the next evening, as she lay on the sofa while her bed was making, she felt a solid substance passing from the rectum, which alarmed her terribly. It was found to be a rope of sloughy stuff, soft and purulent outside, but tough and fibrous within, not unlike the ischiatic nerve in a decayed state, suspended from the rectum for the length of a foot or more. On attempting to draw it away, it appeared to be still adherent within the gut, and she complained of pain. After a little, however, it was removed without much effort, and a gush of matter to the amount of perhaps two tablespoonsfuls followed. The slough was about the thickness of the thumb, or more, and was fifteen or sixteen inches in length. We at first supposed it was a portion of the small intestine which had mortified, and been thrown off; but on close inspection no distinct traces of a canal could be found. Sometime after an injection of warm water and sweet oil was administered, which came away in about twenty minutes mixed with some matter, but without any appearance of fæces. On examining the rectum, a rugged irregular edge was felt at the posterior side, close to the sacrum, as if it was the termination of the part from which the slough had been cast off; the examination gave much pain, especially when the intestine was pressed upon within. Several days passed without much alteration in the case, there was matter daily discharging to the amount of three or four ounces, and there was at times severe dysury, at last demanding the use of the catheter. The urethra was blocked up with a thick mucous discharge, which closed the common instruments, and prevented the water escaping until a very large sized flexible one was introduced. Sometimes the difficulty appeared to be connected with mere nervous irritation, as she occasionally got sudden and unaccountable relief, the water coming off without any very obvious reason. Three days had now elapsed since the subsidence of the pain and tenderness of abdomen, *and six days since the bowels had beed moved.*— There appeared to be some fulness of the abdomen, and she began to feel uneasiness again in the left iliac region; we had allowed the bowels to remain so long at rest with a view to

the healing of the ulceration in the rectum, and from an apprehension that much disturbance of the intestine might increase the inflammation and sloughing. It was evident however, that there might be considerable risk in allowing the distention to go on further. A dose of castor oil was therefore given, followed by pills of aloes and henbane, every second hour, which operated freely in the course of the day (November the 8th); the motions were thin, dark, and streaked with matter on the surface. The pain in the left iliac region nevertheless, to our great surprise and alarm, became progressively worse, and extended rapidly over the whole of the abdomen; there was a return of the restlessness and distress of countenance, and the stomach began to reject every thing, The case now presented a more alarming aspect than it did even on the former attack; there was increasing distention of the abdomen, the pulse became feeble and rapid, the thirst extreme, the vomiting frequent, the countenance was sunk, the look anxious, and the face was covered with a clammy perspiration. We were here placed in one of those difficult situations in which the diversity of medical opinions on the treatment is most distressing to the practitioner; after all his reading and study of the subject, throwing him back without clue or guide upon his own resources. There was too much debility, and the complaint was too far advanced to venture on general blood-letting; leeching alone could effect little; and purgatives, which operated freely both at the commencement of the first attack and of the present, not only without relief, but with apparent disadvantage, might only increase irritation and render recovery hopeless. The recurrence of the inflammation must have arisen either from the slough having penetrated the intestine and allowed its contents to escape into the cavity of the abdomen, or from the direct effect of the purgative; or it might be from our having deferred it too long, and permitted the bowels to become injuriously distended. We thought it exceedingly probable, from the extent and depth of the slough at the time it was cast off, that perforation would eventually take place, and as that event is not always announced by the sudden pain usually ascribed to it, there was reason for strong suspicion of its having occurred. On the other hand, the fact that inflammation of the bowels has often re-

curred from the imprudent administration of purgatives,* and that in this case it had only increased after free evacuations, gave some colour to our apprehensions that they had done mischief.—The argument was, after all, perhaps equally strong in favour of the third and opposite proposition, that the distention of bowels had been allowed to go on to an injurious extent. We had determined, in fact, so great was our apprehension of increasing the slough, and thus occasioning perforation of the intestine, not to give a purgative until we had some manifest indication that the confinement of the canal was doing harm, and hence necessarily the uneasiness of bowels commenced before the purgatives were given. All, therefore, that could be fairly inferred from the continuance and increase of the inflammation after their administration was, that they had failed to arrest it. The result of these considerations was, our trusting to opiates (which had been so successful before) for removing the inflammation, and our determining to watch the progress of the case, that we might, if possible, detect the real cause of its recurrence. Three grains of opium were given at first, and two every second hour afterwards: a dozen leeches applied to the abdomen, and fomentations with decoction of poppy heads, were made use of. The effect was wonderful; the pain and tenderness gradually subsided, the vomiting ceased, the pulse became slower, and she got some sleep. In the morning (November the 9th) the improvement was more considerable; the pulse fell to 110; she retained every thing on her stomach, and her countenance was full of hope and cheerfulness; the tenderness of the abdomen had almost entirely subsided. This perfect relief without the use of any purgative was little calculated to relieve us from our perplexity as to future treatment. She made little complaint for the ensuing five or six days, except that she was occasionally annoyed with dysury; and the continual passing of matter from the rectum, with pain and soreness inside the sacrum, as she described it, which made it distressing to her to cough, or laugh, or sit up in the bed to take food. After the great amendment on the 9th, the opium was gradually diminished, at first to a grain every third hour, and eventually to a grain and a half or two grains at bedtime only. The appetite had returned, the tongue was clean,

* Abercrombie furnishes instances of this, and more lately Dr. Stokes, of Dublin.—See Dublin Medical Journal, vol. i. p. 128.

and she took a fair quantity of nourishment daily, chiefly gruel or steeped bread. We now watched the state of the abdomen with much anxiety, and especially about the left iliac region, where the pain commenced before, and where indeed the injurious effects of distention were likely to be first felt, as in all probability the sloughing extended as far as the sigmoid flexure of the colon. There was already a considerable fulness, but as there was no tenderness whatsoever on pressure, and as we had just a chance that a natural motion might take place, we still refrained from giving a purgative. About the seventh day from that on which the bowels were last moved, the tenderness in the left ilium, as we anticipated, was again felt, and it was soon followed by pain and feverishness, with a disposition to vomit.—There was now no doubt on our minds that the recurrence of the attack was attributable to distention, and not to perforation of the intestine, as we had apprehended. After giving a large opiate, therefore, she was ordered a few grains of calomel, with mild doses of castor oil and jalap every second hour, until the bowels were freely moved. Great relief was obtained, but the pain and tenderness of the abdomen finally subsided on resuming the opiates for twelve or fourteen hours after the purgative had ceased operating.

Although she was now for the third time freed from all symptoms of active inflammation, we could not but consider her situation as very precarious. One should be very sanguine to look forward with any confidence to the healing of such an extensive sore as remained after the detachment of a slough of fifteen or sixteen inches in length, and from which still matter was daily discharged, when the most minute ulcerations of the rectum are so troublesome and difficult of cure. While it was considered an object, under these circumstances, to keep the bowels as long as one safely could without acting, care was taken that no injurious accumulation should again occur. A mild purgative was given at the farthest on every fourth day, which operated without creating pain or uneasiness, and by diminishing the interval gradually, the bowels were after a little brought to act daily with a small dose of infusion of rhubard and cascarilla. She was still however unable to sit up in the bed, or to turn to either side on account of the excessive soreness inside the sacrum : the motions continued to be smeared with matter ; sometimes small bits of fresh slough came

away; sometimes spoonfuls of healthy pus with stuff like jelly. Weak sulphate of zinc injections, and even those of simple water, were made use of, but they gave great uneasiness, and served to do more harm than good. At this time, about eight weeks from the commencement of the attack, she became very hysterical; got fits of crying and laughter, which lasted for hours, and was sometimes slightly delirious. She had been kept very low all through her illness, but was now allowed nourishing diet, meat and a little wine; there was an immediate improvement in all the symptoms; her strength and health mended; her mind became cheerful; the discharge of matter diminished, and at last was only occasionally observable. The soreness about the sacrum was also lessened so considerably, that she was able to dress, lie on the sofa, and sometimes sit up for as a short time. At the end of three months she could move about the room a little, and at the termination of the fourth, she was perfectly recovered, and able to walk some miles in the country without injury or fatigue.

The occurrence of sloughing of the rectum, and perhaps of the colon in the foregoing interesting case, to an extent I believe unparalleled in the records of medicine, created much embarrassment and difficulty in its management. The treatment cannot therefore be reviewed as applying to a case of simple enteritis, without reference to the incidental dangers with which it was complicated. The reader's attention may be fairly directed to some very important facts, and the inferences deducible from them, which bear distinctly upon the general principles of cure in the complaint, and first of blood-letting.

I shall not waste time in debating whether this remedy is advantageous in the early stage of enteric inflammation, any more than I should in a similar affection of any other viscus. It is, I believe, universally admitted at the present day, that blood-letting and the amount to which it may be carried, should bear reference only to the strength of the patient: it is also the practice to estimate the strength by the state of the patient's health previous to the attack, and not at its commencement. A strong man, for instance, may be seized with enteritis, the powers of life may be suddenly depressed, and the pulse become feeble. This is obviously a state of indirect debility; there is no outgoing; nothing in the time to account for it; the man will bear bleeding well, and become stronger as the blood is taken away

from him. On the other hand, a strong woman may become exhausted in a tedious labour; she may be delivered with instruments; she may get peritoneal inflammation. Here if we find a very feeble rapid pulse and other appearances of debility, we cannot with the same readiness pronounce it to be indirect; for the patient has been probably worn out before the inflammation began, would sink under a large bleeding, and must be treated by other means. In the case before us, it will be observed, that the lady fainted at the first bleeding, when eight ounces were drawn, and so little could be obtained afterwards by the lancet, that leeches were resorted to; yet the inflammation went on, and next morning twenty ounces were taken from the arm, and on the same evening eighteen more, without inducing the slightest symptom of weakness. The debility in the first instance was evidently indirect, and probably depended on some nervous idiosyncracy. Occurrences of this kind would lead one to doubt the propriety of always placing the patient upright in the bed, as Dr. Elliotson directs in taking blood. His object is to insure its producing some effect on the arterial system; to bring on syncope: but it is evident, it may bring it on sooner than it would be at all desireable in habits particularly constituted. When syncope occurs without any direct relation to the loss of blood, the effect may be of use in colic; but in violent inflammation it is temporary only; as soon as the heart recovers itself it has yet all the material for maintaining its action strongly, and the inflammation consequently revives with it. There must be an absolute abstraction of blood capable of directly depressing the whole system to produce any permanent influence on the disease. Dr. Marshall Hall's ingenious conjecture, that the amount of inflammatory action might be estimated by the power the system evinces of resisting syncope in the erect position, is only true in the advanced stages of inflammation, when all the idiosyncrasies are sunk or lost in the one great absorbing action. In the commencement the character of the habit prevails; in the advance, the character of the inflammation only. At first we shall have differences in the symptoms in different individuals, though the disease be the same: by and by these disappear as the inflammation rises, and it displays an identity of symptoms in all. Dr. Marshall Hall's test of inflammation will, therefore, we fear, be often found unavailing when assistance in

the diagnosis is most needed; that is, in the beginning. In the case, the history of which I have detailed, the lady, though healthy, and of a full person, was of a very hysterical habit, just such a one as those attacks of simulated inflammation might readily arise in. The occurrence of syncope on withdrawing a few ounces of blood, made it still more probable, that it belonged to the class of irritative disorders, rather than to those of a purely inflammatory nature. But on examining the spinal column to ascertain whether there was any morbid state of the cord to which the excessive tenderness and pain of the abdomen could be ascribed—*there was no soreness on pressure.* Notwithstanding the fainting, I had, therefore no hesitation in looking on the attack as one of severe inflammation, and in this I was amply borne out by the event.

But to return to our subject; no one now, as I have said doubts the propriety of bleeding as largely as the strength will allow in the early stage of enteritis, or that the earlier the lancet is made use of the better. But the question remains, shall we not bleed at any period of the disease while the inflammation exists, limited only, as in the early stage, by the powers of the constitution? It is said, and as I have shewn by high authority, that bleeding, if it should not succeed, and its success is of course very doubtful in the advance of the disease, hastens the stage of gangrene; this, it must be admitted, is very contrary to what occurs in the inflammations of other organs, and to the very prevalent belief among medical men, that gangrene in these instances results purely from the violence of vascular action. If the tendency to it arises from a peculiar diathesis existing at the time, bleeding can of course do no good, and may possibly prove injurious; but what remedy in such a case could save the patient. If on the other hand, it supervenes from the violence of the inflammation, what can prevent it if bleeding does not? Though disposed, as far as my own views are concerned, to reason in this manner, I can by no means treat slightly the apprehensions of those able physicians who have condemned the use of blood-letting in protracted enteritis. Their opinions, however erroneous, must have been founded on the experience of its ill success, and before we can fully reconcile ourselves to the practice, this ill success should be accounted for. It will be allowed on all hands, that one large bleeding has often subdued this and other danger-

ous inflammations, when employed at an early period; while depletion to any amount scarcely succeeds, if it has lasted some ten or twenty hours. There is, then, evidently, even with the same apparent amount of inflammation, a considerable difference in the actual pathological condition of the system at the end of the first, and of the second, or third day; there is some change of state or structure produced capable of keeping up the inflammation, which did not exist within the first twenty-four hours; the inflamed part has acquired a vitality beyond that of other organs, and as bleeding only subdues inflammation by diminishing the vital powers generally, those organs will necessarily die before the inflammation can be thus subdued. It follows, therefore, if the indication with which we commence depletion, that of absolutely subduing the disease, by abstracting from the powers of the system at large, should direct us all through its progress, we should, after a very short period, be aiming at what was impossible of accomplishment, and very often exhaust the powers of life beyond the hope of recovery. It was this error we apprehend, that occasioned all the ill success which made such an impression on the older practitioners, and brought a most valuable remedy into unmerited disrepute. The truth seems to be, that when we have failed in arresting the inflammation by free and early bleeding, we must give it up as our main resource, and employ it as auxiliary only. We may still make use of it, but it should be simply with the view of diminishing or breaking the violence of the disease, and rendering it more amenable to remedies which operate after a different manner: to leeching, which, by drawing the blood from the inflamed parts directly, subdues the disease in a far greater ratio than it subdues the living powers generally; to opiates, which, in diminishing the sensibility of the system, seem also to lessen its power or tendency to support inflammation; to mercurials, which counteract its action; or to counter-irritation, which relieves by derivation to some less important tissue. This was the doctrine, or at least the treatment of Armstrong, and that his admirable papers on inflammation have as yet had so little influence on general practise, is to us one of the wonders of modern medicine.

I now come to consider a question of much greater difficulty and about which practitioners are almost as much divided in opinion at the present, as at any former period; the employment of

purgatives in this disease. It is an undeniable fact that in a great number of cases of enteritis, the salutary termination of the camplaint has been by free evacuation from the bowels, and that before this occurs perfect relief is seldom obtained. To the experience of this strong fact, we may readily refer the popularity of the purgative treatment, and indeed, it would seem almost extraordinary, that other and so different modes of management should have made any way in opposition to it. Those instances, however, of occasional occurrence, in which the complaint went on to a fatal termination, although the bowels were free or easily moved through the whole course of it, or those in which perfect relief was obtained, as in the one I have given, without any discharge at all by the bowels, necessarily startled the practitioner, and led him to inquire whether he had not been too hasty in generalizing his conclusions. Effects are too often, in the science of medicine, mistaken for causes. When cholera first appeared in this city, calomel was profusely employed in its cure, and it was eventually found, that patients who became salivated almost invariably recovered. This was esteemed proof positive of the efficacy of the treatment, and mercurials became more popular than ever. I found, however, on examining the registries of the hospital with which I was connected at the termination of a month, that in the stage of collapse no more than one patient in ten could be brought under the influence of mercury, so that there were only four recoveries in forty.*—This told little for the remedy, as far as cases in collapse were concerned, and I immediately set about ascertaining what the amount of spontaneous recoveries might be in the same stage.—From all I could gather from the experience of others or my own, I began to suspect that they would reach nearly the same amount, and at last I became perfectly convinced, that the actual fact was, *the patients did not recover because they were salivated, but they were salivated because they recovered.* Mercury in any shape, in the stage of collapse, was thenceforward discarded from my practice in the hospital, and though it excited some observation at the time, the subsequent experience of the profession at large bore me out in

* The same registries proved the decided efficacy of calomel, at any period of cholera previous to collapse. Of those patients who were brought in with the pulse perceptible at the wrist, we sometimes lost but five in the hundred, and never during the most fatal period more than fifteen.

the decision. I cannot but feel, that somewhat of the same error prevails with respect to purgatives in enteritis ; the disease is not a very common one, and the experience of an individual could scarcely warrant him in offering opinions at all confidently, when they are opposed to general practice; but certainly all the information I can glean, or the experience which has fallen to my share, would dispose me to say, that in intestinal inflammation, the relief obtained is seldom the direct effect of the purgative, and that *people do not recover because they are purged, but they are purged because they recover.* I shall, however, examine some other arguments that have been offered in favour of this treatment.

"For what reason," inquires a reviewer of Dr. Abercrombie's opinions on the subject,† "do we employ active purgatives in the early stage of thoracic inflammation? To lessen the whole mass of circulating fluids, and reduce the general action of the heart and vascular system. Now, in abdominal inflammations provided the mucous tissues are not inflamed, purgative medicines excite the secreting vessels, not only of the whole internal surface of the intestines themselves, but of the glandular organs whose excretory ducts open into the primæ viæ, and thus powerfully deplete locally the vascular system of the abdominal viscera." Again it is said, "when that portion of the peritoneum reflected over the intestines is inflamed, and the villous coat unaffected, to excite the natural action of the mucous membrane, immediately after proper vascular depletion, is a powerful means of checking the disease ; in the same way, that a free expectoration from the mucous membrane of the lungs, relieves the vascular turgesence and inflammation of the parenchymatous structure or pleural covering of the same organ."

With respect to the first argument it is sufficient to observe, that no one can dispute the utility of the indication, or that purgatives would tend to fulfil it, in any inflammation in which the bowels themselves were not engaged. But it is a different matter attempting to lessen the quantity of the circulating fluids, and diminish vascular action, by stimulating healthy secreting surfaces to action, at a distance from the inflamed parts ; and stimulating the inflamed part itself, or parts contiguous to it.—All analogy is against the principle of exciting the action of in-

† See Medico-Chirurgical Review for September, 1820.

flamed tissues; the only exception to which, that I can bring to mind, is to be found in the inflammation of mucous membranes. These, it is ascertained beyond doubt, are often more readily cured by the application of strong stimulants of a particular class after depletion, or sometimes without it, than by the usual antiphlogistic treatment. An ointment of strong nitrate of silver is found to cure the purulent ophthalmy of children and indeed a large proportion of all simple inflammations of the mucous membrane of the eye, more rapidly and permanently than purging and leeching. An injection of the same preparation is said to subdue recent gonorrhœa more speedily than other treatment, and we have indisputable testimony of the advantage derived in dysentery from the use of purgatives, although they have been so deprecated by the Broussaists, and indeed by those of a much less theoretic school. The advocates for purgatives in peritoneal enteritis, adopt this principle, but they adopt it too literally, and without sufficient reference to actual results. They are strictly for adherence to the rule of leaving inflamed parts at rest, without making any exception in favour of mucous membranes, and their doctrine, consequently, is, to purge when the peritoneal coat only is affected, as in enteritis, not to purge when the mucous coat is the seat of disease, as in diarrhœa or dysentery, In both I differ from them; I have shewn that inflammation of mucous surfaces cannot be brought precisely within this law, which seems to influence all the other tissues; and with respect to the propriety of purging in enteritis, because the mucous coat is free, it appears to be introducing a refinement in practice, which the results will not bear us out in. It is absolutely true that we can violently stimulate a mucous surface which is healthy, in connexion with a muscular or serous which is inflamed, without increasing the inflammatory actions in the latter? I believe not: on the contrary, to refer again to the eye, where we can see the thing, we find that the same stimulants, that in a state of simple inflammation of the conjunctiva produced a rapid cure, will, if there be any inflammation of the sclerotic coat, make both textures worse. Mr. Guthrie is never in the habit of using the nitrate of silver ointment when any of the internal coats of the eye are engaged in the inflammation, although if the theory I have been objecting to was correct, stimulating the conjunctiva to increased secretion, ought to relieve the sclerotic. It seems

in fact, not only true, that it is injurious to excite parts acutely inflamed into action, but it is not safe to stimulate parts which are contiguous or closely connected with them; and this is commonly held in view in the very instance which the writer before referred to cites as an analogy, that of inflammation of the lungs or pleura. It is never attempted to stimulate the vessels of the bronchi to increased action, until the inflammation of the lungs is subsiding under the influence of remedies which operate in a different manner, as blood-letting, emetic tartar, calomel and opium or blistering. It is indeed well known, that efforts to bring on expectoration in acute pneumonia previous to large depletion will always fail, while it sets in almost spontaneously if the inflammation is first got under controul. In the same manner it is almost univerally found, that purgatives will not operate in the early stage of enteritis, unless very free depletion be made use of; but that when depletion is premised, and the force of the inflammation broken, motions will be found to come on naturally, or with the assistance of the mildest medicines. Thus what are said to be most important parts of the treatment in both instances, expectorants and purgatives, are absolutely unavailing or injurious when most required; that is, when the inflammation is at its highest point, and disorganizations rapidly proceeding. This cannot be said of the remedies which have been preferred by those who are indisposed to the employment of purgatives in the early stage of enteritis, as leeching, opium, or calomel and opium; they are useful from the first, and would in numerous instances effect cures without blood-letting at all, or where it had failed, or was totally inadmissible.

To revert again to the case of enteritis, the history of which I have given above, there was an accidental peculiarity in the management, arising out of our apprehension of the sloughing, which led to some interesting results. We not only avoided purging, but for a time designedly kept the bowels at rest that there might be opportunity given for reparation. If purgatives were so essential, this would have been fatal; yet in one instance the bowels were unmoved for six days, and in another, seven days, before injurious distention took place. In the third, where they were allowed to remain confined to the fourth day only, no injury at all was sustained. *On all these occasions purgatives had been given at the commencement of the inflammation, long*

before it had reached an alarming amount, and they had operated freely without giving the slightest relief. If they had been persevered in, instead of the opium treatment, will any physician take upon him to say, that the termination would not have been fatal? or that any other remedial plan would have procured such rapid amendment and with such apparent certainty?

It is not a little in favour of the opium treatment, as contrasted with that by purgatives, that Dr, Armstrong scarcely alludes to the latter as a remedy, although he does not seem to have had any more decided objection to them, than their uncertainty and inferiority. Whenever he thinks of comparing the effects of opium with those of any other remedy, it is only with blood-letting.—He tells us, he has witnessed some cases of inflammation of the bowels, where full doses of opium finally effected the cure, after bleeding and purging had completely disappointed his expectations. "So great indeed," he says, "is my confidence in full doses of opium in peritoneal enteritis, that if compelled to say, supposing myself the subject of the disorder, whether I would exclusively rely upon them solely, or upon blood-letting solely, I should certainly fix upon the former; at the same time I should like to have the simultaneous influence of both remedies, being convinced they are by far more serviceable combinedly, than separately employed."*

This was supposed to be a somewhat extravagant encomium on opium by Dr. Armstrong at the time. Since the death of that celebrated and excellent physician, Drs. Graves and Stokes of Dublin, have directed our attention to cases not of very unfre-

* Dr. Armstrong first invariably bleeds to approaching syncope, whatever may be the quantity necessary to produce this effect. As soon as the patient revives, three grains at least of good opium, in the form of a soft pill, are given, and quietude is strictly enjoined, so that if possible sleep may be obtained In some irritable habits less of the solid, and some fluid opium are prescribed, that the anodyne and sedative effects may be more quickly produced. Its effects, he tells us, thus administered, are to prevent a subsequent increase in the force or frequency of the heart's action, and a return of the abdominal pain, while it induces a tendency to quiet sleep, and a copious perspiration over the whole surface. In many instances this simple procedure will remove the inflammation at once, nothing being necessary, when the patient awakes, but spare diet, absolute rest and quietness, with an occasional mild laxative. He always visits the patients in three

quent occurrence, in which there is so much debility connected with inflammation, that one cannot look for advantage from depletion in any shape; such are the cases occurring after tapping in exhausted dropsical habits, and of low puerperal peritonitis.—These physicians have also given some remarkable ones of peritoneal inflammation from perforation of the intestine, in which not only was bleeding altogether forbidden, but the operation of a purgative would have been certain death. These were in a worse alternative than that which Dr. Armstrong supposes for himself; there was no choice left them but opium, and it in every instance surpassed all the anticipations he could have formed of its unaided virtue.*

Perhaps no little creature was ever restored to life and health again, after having approached the verge of existence so closely, as a child, who was under my care about two years since—the following are the notes taken of the case.

Case II.—A delicate girl, aged ten years, who had only been six or eight months recovered from a tedious and most alarming

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or four hours after giving the opium, and if there be pain on pressure in any part of the abdomen, with a hot skin, and quick jerky pulse, he bleeds again to complete relaxation, and repeats two grains of opium with four or five of calomel, in the form of a pill, as the faintness disappears. A third venesection is rarely requisite, but if, after the expiration of five or six hours, pain and fever still exist, it should be performed once more, and followed by the administration of a grain of opium, and two or three of calomel immediately, and half a grain of opium and two of calomel every four hours, until sleep and general perspiration are induced.

It is evident from the cases published by Drs. Graves and Stokes, and from the one above detailed, that the opium may be safely employed with much more freedom than Dr. Armstrong was accustomed to recommend. There is one precaution which it may perhaps be necessary to offer: I have two or three times found difficulty of passing water succeed the opium treatment, where it was perfectly successful, and in one instance, after a profound sleep, the patient was awakened by uneasiness from distended bladder, and could not evacuate it at all; the catheter was introduced, and gave instant relief. When calomel is conjoined with opium Dr. Armstrong very properly reminds us, that as the specific effects of mercury are most easily produced after copious abstractions of blood, we should use a proportionate care in its exhibition.

* Dub. Hospital Reports, Vol. v.; also Dublin Med. Journal, May, 1832.

attack of chronic mesenteric disease, came in sick from the garden after having gorged herself with every kind of fruit. She took some tea and went to bed, but was awoke out of her sleep in two hours with violent pain in the stomach and side and disposition to vomit. She got warm water and discharged quantities of gooseberries and currants off her stomach, which had become quite tender to the touch. As the pain continued she was afterwards put into a hot bath and got castor oil and laudanum, after which she became easier and went to sleep. I saw her at twelve o'clock on that night. She was then resting very quietly—her skin cool and her pulse ninety. As very judicious remedies had been already employed, I thought it would be wrong to awaken her, although her countenance looked badly. She slept pretty well, but awakening, as I was informed, at five or six o'clock, got more castor oil, which had not operated at nine, when I came to see her. She was now alarmingly altered—her countenance was sunk and anxious, her pulse feeble at 130, the abdomen swelled and exquisitely tender, especially at the left side midway between the short rib and crista of the ilium, and the stomach inclined to vomit every thing. There was no spinal tenderness. She got two pills of aloes and extract of henbane, with five grains of calomel, and the former were directed to be repeated every two hours; an enema was also given, but it brought away nothing, and sixteen leeches were afterwards applied as near as possible to the spot were the tenderness of abdomen was most acute. I was afraid to bleed generally in consequence of the great feebleness of pulse, although the inflammation was evidently of a most intense character. The sickness of stomach and vomiting came on always after drinking, and there was great restlessness and tossing in the bed. As the leeches continued to draw blood the weakness of pulse and restlessness seemed to encrease, the forehead became cold and clammy, the eyes looked sunk and the face drawn. I removed the leeches instantly, (they had been on about twenty minutes or perhaps half an hour) and closed the wounds with starch—meantime I gave her a teaspoonful or two of wine at intervals. The restlessness and sinking, however, still continued—she threw herself from side to side frequently, and could not be got to keep her hands quiet; the pulse was 150 and very feeble, and the vomiting still recurred as soon as a few teaspoonfuls of wine were taken. Altogether she presented just

the appearance and manner of a woman dying of hemorrhage.—She did not complain much of the pain, although the tension and exquisite soreness of abdomen continued. I now gave her teaspoonfuls of burnt brandy with ten drops of laudanum instead of the wine, and as I was exceedingly alarmed about her, requested the assistance of another medical gentleman. The stimulants although they did not remain long on the stomach, seemed to be of some service, as she rallied a little before the physician who was sent for arrived. The restlessness was less and the stomach quieter. A simple enema was now given and the brandy continued. In about an hour, another enema was given with some turpentine. Both the injections came away together and brought off a quantity of currants and gooseberries, but no fæces. During the night the burnt brandy a little diluted, and sometimes wine was given. An assafætida enema was administered twice but passed away after a considerable time without bringing off anything, and she got a little chicken broth. The restlessness and vomiting, however, still came on occasionally, the abdomen was rather more distended and exquisitely painful to the touch, so that she always cried when we approached to examine it—the bowels were not moved, there was some hiccup now and then, and the pulse was feeble at 160. She did not, however, complain of pain nor appear altogether so sunk as on the evening before, although she had got no sleep. This was the second morning since the commencement of the inflammation. I had been anxious from the first to give large opiates in this case, those I administered having produced no sensible effect. The small doses given could, in fact, be of no avail, as she usually vomited every ten or fifteen minutes or oftener, precluding the possibility of any accumulated effect from them. It was, therefore, necessary to give a large dose at once, if any decided benefit was to be attained by the remedy at all. This, however, was objected to by the Physician in consultation. The bowels, which were tense and somewhat tympanitic, had not been moved since the inflammation commenced, and it was feared that a large opiate might *lock them up* permanently. It was considered preferable to give some aloes with croton oil in pills, on the supposition that any chance there was of recovery depended on getting the bowels freed. I suggested the danger that appeared in my view of the case in giving any purgative at all, and especially croton oil, while, if the opiate produced any effect, it would be

most likely that of relieving the inflammation which would be a step towards recovery, whether the bowels were moved or not. As, however, the gentleman with whom I consulted, was a man of great experience and ability, as he was decided about the correctness of his opinion, and as I had then no experience of the effects of large opiates in a case precisely similar, I consented to his proposal, and two pills containing a few grains of aloes and quarter of a drop of croton oil were given to the girl. Very soon after she became much worse and towards noon the sickness of stomach, hiccup and prostration of strength became frightful. The pulse was scarcely perceptible at 160 or 170—the face blanched and covered with cold perspiration, the eyes sunk like a person moribund. The poor little sufferer flung herself from side to side, calling for breath, and continually throwing her clammy hands about restlessly. This state alternated with the vomiting, which, severe as it was, appeared the less distressing of the two to those who stood by. I had been feeding her with brandy and wine by teaspoonfuls every four or five minutes for the last hour or two. She took, I think, two or three ounces of white wine, and perhaps half a glass of brandy burnt within that time, with no perceptible effect. Convinced that she must now die unless opium gave relief, I measured twenty drops of laudanum into a little brandy and gave it to her. It appeared, however, to have no effect, for the feeling of sinking continued, and I once or twice heard the little creature utter in a faint voice, "I'll die Doctor—I cannot live till morning." She also told her father that she was going from him, and a scene took place that it is needless here to describe, but which could never be forgotten by any one who witnessed it. I just waited until half an hour had elapsed before repeating the sedative, and then gave a solid grain of opium in preference to the laudanum—a large dose as a repetition to a child only ten years old. When another quarter of an hour had elapsed I watched with intense anxiety, and, I confess, with profound despondency for the effect; but the tossing and restlessness and vomiting went on, and I expected every now and then to see her rise up, fling herself back upon the pillow and expire. Ten minutes more passed and there was a languor in her movements and less distress in her breathing, and when the half hour was gone by she was lying still and inanimate. I hoped the opium was influencing the nervous system.

I stood looking silently and intently at her unusual quietness on the pillow, and while wondering whether it was to be attributed to the antagonizing power of this extraordinary medicine, my little patient was asleep. Physicians have wearying lives; they have more anxiety, more responsibility, than any other professional men, and they have continually to witness pain which they have no power of allaying, and scenes of misery which they cannot mitigate—but they enjoy some bright moments which go far to make them amends and which fall to their lot only.—This, to me, was one of them—for I saw that my patient was recovered.

She did not move until eight o'clock the next day, and she then awoke with a smile; she had neither hiccup nor sickness of stomach throughout the night and complained now of no pain. The tenderness and tension of abdomen however, was as great as ever. She took a little tea with a liking. Having witnessed the ill effects of the purgative the day before, we determined to leave the relief of the bowels to nature, and we were contented with applying a bread and water poultice to the abdomen. She passed the day quietly, took liquid nourishment—tea, coffee, or barley water, and slept frequently. In the evening she got a simple enema with five grains of aloes in it, and with this we left her for the night. At about twelve o'clock her bowels were moved, not very loosely as after a physic, but in considerable quantity, and the evacuation was of as natural and healthy a character as it could possibly be. She then got another grain of opium and went off to sleep In the morning her bowels were moved twice naturally and freely, and the soreness of abdomen, though still extreme, was circumscribed to a patch between the left ileum and umbilicus, the very centre of which was excessively tender and had some feeling of fluctuation. This spot in the centre was the point where the inflammation commenced, and was more tender than any other all through. The pulse was now 80, the skin natural—the countenance cheerful and the appetite returning. The opiate at night was continued. On the fifth or sixth day of her illness, when she appeared to be going on very well, an eruption appeared all over her person, exactly resembling mercurial erythema, or the rash which sometimes appears after recovering from cholera, at which I was excessively alarmed; it went off, however, in two or three days without any ill consequence. The

pulse afterwards quickened again; she became a little feverish, and as the abdomen of the left side still continued tender, and we were apprehensive that an abcess might form, a blister was applied. The soreness was considerably less after the healing of the blister, and gradually abated. Eventually she became quite well, and is now a healthy girl.

I have been favoured by my brother with a report of a case scarcely less extraordinary than that just detailed. It tells so strongly for the influence of opium in one of those circumstances of disease to which Drs. Graves and Stokes have pointed it out as so particularly applicable, that I shall venture to quote it.

Case III.—"In the early part of this year I was called to see a boy, five years old, in the last stage of typhus fever. The illness had been very much protracted, and had produced great emaciation and debility. I found him lying partly on his back, and partly on his side in the bed, moaning, speechless, and insensible; the wasting was excessive, and the pulse 140 and feeble, indeed almost imperceptible; his features were quite drawn and pale; his nose, cheeks, and forehead cold; and on examining the feet and hands, they were found equally so; his head had fallen aside on the pillow; his breath was drawn by gasps, and at intervals, some seconds apart, like one expiring. When some drink was put to his mouth with a spoon, it brought on distressing cough from the difficulty of swallowing. On examining his abdomen, which was tumid, hot, and excessively tender all over, he moaned loudly. The only treatment he had been under for some days, was, the use of occasional doses of castor oil which had not operated; his bowels had not been moved for three or four days.

"If there had been more strength, bleeding would have been indicated by the state of the belly, and considering the state of the bowels, confined for three or four days, and now tumid, hot, and tender, I believe, very many practitioners would have been tempted to employ at the least some mild purgatives. I recollected, however, those interesting cases by Drs. Graves and Stokes on the use of opium in cases of perforated intestine, and as ulceration of the mucous membrane, perforation, and peritonitis are not uncommon terminations of typhus fever, and this little fellow's symptoms seemed in a degree to indicate some

such lesions, I ordered him a grain of opium with four of extract of henbane, more as a forlorn hope than with any expectation of its averting the impending dissolution. To my great surprise, he spent the whole night in a profound and quiet sleep. He took scarcely any drink, and the little he did take was given him with difficulty by a spoon; but the tenderness of abdomen and moaning were much less; the pulse slower and more steady; and he shewed some signs of returning sensibility. The opium and henbane were repeated, and a dose of castor oil was given early on the following morning. When I saw him in the course of the day, the improvement was so great, that he could take a cup in his hand to drink, and from this time forward his recovery was certain. The opium was still repeated regularly for a few nights, and when the abdominal tenderness diminished, his bowels were easily moved by castor oil, of which he got a moderate dose daily. As the secretions were found to be much deranged, he was ordered small doses of calomel twice a day for a week or more, at the end of which period he was quite convalescent."

My brother's suspicions of perforation having occurred in this instance, would appear somewhat probable from the fact, that it may and does occur without the violent or sudden pain which has been usually stated as one of the principal symptoms. It would appear from an extraordinary case which lately came under my own observation, that in states of great attenuation and exhaustion of the system, perforation of the intestine with effusion into the abdominal cavity may take place, and continue until death without occasioning any pain or inflammation at all. As it is in same degree connected with our present subject, and for extent of intestinal disease, without the interruption of any of the living functions, is not a little singular, I cannot resist the temptation of offering it to the reader, at the hazard of prolonging this article a little unreasonably.

A fine boy, aged ten years, after a severe attack of measles, during which the pulmonic symptoms were very severe, was affected with occasional pain in the left side of the chest, harassing cough and hectic fever; which after some time were relieved by medical treatment. There remained only a great appearance of delicacy; constant rapidity of pulse, and an incapability of much exertion: even in these respects he seemed to amend con-

siderably during the summer, by change of air, and gentle exercise. The hectic fever and cough, however, recurred again, without any apparent cause, in the commencement of the winter, and there was disorder and irregularity of bowels; with other symptoms of mesenteric disease. He seemed to improve occasionally for short periods, and got on to the next winter. The symptoms of the affection of the chest were now entirely supplanted by the abdominal disease; he had little cough; no pain of chest; but there was a rapidity of pulse with low evening fever, excessive languor and emaciation, with fulness and pain of belly, attended by a loose state of the bowels. When the fulness and tenderness were less than usual, the enlarged mesenteric glands were distinctly felt, especially in the right iliac region. The swelling of these glands progressively increased at the side mentioned; where it was attended with so much pain and tenderness, that leeches were applied. Dr. Marsh, with whom I had consulted on the case at an earlier period, was again advised of the state of our little patient, and seemed to consider that an amount of tuberculer disease had taken place, for the relief of which there was little hope. Besides; the extreme weakness and the increasing irritability of stomach which existed, precluded any treatment beyond palliation of the symptoms. He had now generally from three to five free evacuations in the day, of a light yellow colour, and little consistence, and complained occasionally of pain in the belly, which was sometimes full, and at others very little so; the changes were so frequent in this respect, that they were attributed to the absence or presence of flatulence; but on examining the boy carefully, one morning, I found evident fluctuation. My relative, Doctor Geary, who saw him about the same time, agreed with me, that there was water in the cavity of the abdomen, and we concluded that the case was terminating in a very common mode, by ascites.—In a few days after, on examining the abdomen again, there was little swelling and no fluctuation, and later yet they were once more perceptible. I at last concluded, that the fulness must be the result of mere distention of the intestines with fluid, and that the changes were attributable to my accidentally making the examinations immediately before, or after free evacuation. As the abdomen never afterwards swelled to any greater amount, I attached little importance to the feeling of fluctuation, and did

not repeat my examinations with much accuracy; the tender knots or lumps had latterly altogether subsided, and the abdominal pains were seldom complained of; the looseness of bowels, however, was on the increase; the appetite was fickle and failing; the debility excessive, and there was low fever in the evenings. The little fellow was at this time removed to the country, and I did not see him for some days; he then appeared to me amazingly altered; his countenance had grown sharp and hollow; his voice weak; the motions of his lips tremulous; his pulse was rapid and thread-like; he was unable to sit up or to move about, though dressed daily and taken into the open air, and he was emaciated to the last degree. It was painful to see his naked person, it looked like an anatomy, over which the skin had been drawn. His parents told me, although the motions had become yet thinner that before, than they were unaccompanied by pain; that he had no perspiration; ate a little meat, or a few oysters, every day, and drank some porter; but he was notwithstanding declining rapidly. In a day or two after, as his mother was raising him from the bed to her lap, his countenance altered suddenly, his voice faltered, and he appeared to be fainting; they got him some wine and water, which he attempted to drink when held to his lips, but was unable; he laid his head on his mother's shoulder, gasped like a dying bird and expired.

I have been particular in describing minutely the mode of the child's death in this very interesting case, as a proof of his having sunk from mere exhaustion. The examination of the body took place on the third day after. On laying open the abdomen a pint or more of yellow fluid, very much resembling what he passed by the bowels, for days before, was found in its cavity. On raising a fold of one of the small intestines, a hole sufficient to admit the finger, with curled or thickened edges of a yellow colour, presented itself: it had somewhat the appearance of an opening made with a red hot iron—a look as if it had been burned out, and the same description of yellow fluid was flowing from it, as we found in the general cavity of the abdomen; within two or three inches of this, was another perforation nearly as large, and to our astonishment, on tracing the canal upwards, we discovered perforations through its whole extent, from the termination of the ileum, to within a foot of the duodenum, which last

as well as the large intestines, were free from ulceration. *There were seven-and-twenty holes of various dimensions from some that admitted a quill, to others, more numerous, that a finger or thumb would pass through.* They were quite open ; the yellow fluid described flowing from them, when raised up ; and occasionally, when much handled, more consistent fæces, but of a similar colour. My brother, who assisted me in the examination, agreed with me in the conclusion, that there was no observable difference between the fluids found external to the intestines, and that which flowed afterwards from them, by the perforations. These last had by no means the appearance of having been recently formed ; they were for the most part too large and open to suppose so for a moment, and they were evidently not all of the same age ; some having minute openings with thin, floculent edges, others larger ones and more ragged, and many of the largest with thickened margins, as if of old standing. Independent of these appearances, the vast number of them was much against the snpposition of their having occurred suddenly, as it would be rather too much to believe, that twentv-seven ulcers perforated the intestinal canal in a day or a night, without occasioning the slightest symptom of such an event during life. On examining the intestine on the inner side, the ulcers were found to be of much greater extent than the perforations would lead one to imagine ; the coats appeared to be gradually eaten through, in a less degree at the outer area of the ulcer, and greatest towards its centre, where they at length gave way ; there were many ulcers besides, which had not yet perforated the gut, although in most instances they seemed to have destroyed all but the peritoneal covering. In one only of the perforations had nature made any effort to accomplish a cure ; it was a large one at the lower end of the ilium, and the transit of fæcal matter was prevented by effusion of lymph and agglutination of its edges to the adjoining fold of intestine ; the adhesion was very slight and readily broken through.

The mesenteric glands were considerably enlarged, some had suppurated in the right iliac region, and were filled with scrofulous looking matter. Two large absesses filled with matter were also found near the root of the left lung ; through the substance of which minute tubercles were scattered; the right lung was healthy ; the brain was not examined.

How long the perforation had existed previous to death in this interesting case, and whether the liquid contents of the intestines passed and re-passed through the ulcerated holes continually, are questions which must be solved by future observation. It is sufficient for us at present to consider the extent to which, in certain states of the system, a disorganizing complaint of this kind may go forward without evincing any marked symptoms by which it could be detected. We may at least deduce this inference from it, that in enteric inflammations occurring in exhausted habits, if preceded by diarrhœa or other marks of affection of the mucous membrane, perforation may take place without much or sudden pain. It is, therefore, necessary for us to be particularly vigilant and cautious in our treatment under such circumstances.

I shall offer only two cases more in illustration of the efficacy of opium in abdominal inflammation. They are, I think, as convincing as it is possible for any cases to be.

Case IV.—A strong active man, aged thirty years, was seized with pains in the abdomen, chiefly about the situation of the umbilicus—the pain came on more violently at short intervals, but never ceased and was attended with extreme soreness of the abdomen, especially in the situation of the pain—the stomach was sick. He got castor oil and turpentine which was vomited up again. He afterwards got repeated doses of calomel and colocynth with injections, but the former produced no effect and the latter came away as they were administered without any admixture of fæces. He was subsequently blooded to the amount of thirty ounces or upwards, with some little relief to the pain and the purgatives were continued, but after some short time all the symptoms became worse. About ten o'clock at night the patient's friend became exceedingly alarmed at his increasing illness, and desired a consultation, upon which occasion I was requested to see him.

I found him lying on his back, moaning faintly, and complaining of constant pain about the umbilicus, which, at intervals of a few minutes, encreased to a violent degree—he had frequent retching, and could retain nothing solid or fluid upon the stomach—his countenance was dejected—his pulse 100—his skin rather warmer than natural. He had had no movement of the bowels since the commencement of the attack. The centre of the abdo-

men was covered with a blister so that I could make no examination as to the degree of tenderness, but all the parts above, below and at the sides beyond the margin of the plaster, were excessively sore to the touch. The medical gentleman in attendance finding all his efforts to get the bowels moved were unavailing, was just preparing to give him some croton oil.

I represented to him, how very unlikely it was, that by any purgatives he could get an inflamed bowel to act—that inflamed muscles never do. That the chief object appeared to me to be to subdue the pain and inflammation, leaving the evacuation of the bowels entirely to nature, which would probably effect all that was desirable when not interfered with by an inflammatory condition of the parts. I proposed that the blister which had been on only about two hours should be removed, and that eighteen or twenty leeches should be applied about the umbilicus where the pain was most acute—that warm fomentations should be afterwards applied both to allay the suffering and encourage bleeding from the leech-bites, and that two grains of opium with two of calomel should be given immediately, with one grain of each every hour afterwards until the patient fell asleep. On the medical attendant expressing some alarm at the quantity of opium which might be given in this way, and the bowels obstinately constipated, I observed, that if sleep could be procured, the man would probably awaken freed from all pain, and have his bowels moved without the necessity of a purgative.

The leeches were applied and the bites bled freely with the fomentations. The opium remained on the stomach, and soon after the patient took the third dose the pain evidently abated.—In less than an hour he became easy, had a heavy look, and before the hour came round for the next dose was in a sound sleep. At seven o'clock in the morning, after six or seven hours uninterrupted sleep, he awoke freed from pain or sickness, and on being assisted to the night chair, had a free, easy, and copious evacuation. He had one or two more motions in the course of the morning and required no further treatment.

In two days after, this patient had a slight recurrence of the pain from imprudently exposing himself to cold by going into the open air. His medical attendant ordered him castor oil—the pain, however, continued, and was hourly on the increase until he re-

curred to the opium and calomel, of each of which he gave two grains with the same result as before.

Had the ordinary purgative treatment been adopted in this case, I feel convinced it would have gone on from bad to worse, and that at the end of two or three days the bowels still unmoved would have become tympanitic or gangrenous.

Case V.—A woman, aged 30 years, and about six months pregnant, was seized with violent pain between the short ribs and ileum at the right side of the abdomen, extending to the umbilicus. The pain was excessively severe, and came on in paroxysms —the bowels were confined. She took medicine—got an enema and was stupid without the least relief. After suffering in this way for three days, a medical man was sent for, who immediately bled her and ordered her purgative medicine. The latter was thrown up, but some castor oil which she took afterwards remained on her stomach and purged her freely. There was, however, little or no relief, and after the lapse of two days more of intense suffering, I was sent for to visit her. On my arrival I found her lying on her back in the bed with panting respiration, at about 40 in the minute, and constant moaning. She could not bear to be stirred in the slightest degree, or to move her lower limbs, in consequence of the soreness of abdomen, nor had her bed been made for some days. The slightest pressure on any part of the abdomen—the least attempt at turning on her side, or even the motion of her lower limbs, occasioned excruciating pain, and obliged her to scream out. The pain became more violent by paroxysms, though never subsiding, and the tenderness was more acute on the right side where the affection commenced than at any other part of the abdomen—The pulse was about 120—the tongue coated, and she complained of thirst, but there was no considerable sickness of stomach, although she occasionally rejected her medicine.

This case presented altogether so unfavourable an appearance, that I entertained very little hope of doing any good. She had been totally neglected at first. The bleeding was not repeated as it ought to have been, and the castor oil, though operating freely, gave no relief. A general bleeding appeared to me to be now a dangerous remedy, as the vital powers were worn down by the long protracted suffering, and considerable organic mischief had already taken place. A grain and a half of opium with two

grains of calomel were directed to be given every half hour for two doses, then a grain of the former with two of the latter every hour until she should either feel relief from the pain or fall asleep, and eighteen leeches were applied to the right side, followed by fomentations, and finally a gruel poultice. After taking nine pills in the course of the night, she got ease and fell into a quiet doze towards morning. This lasted for several hours, but towards evening there was a recurrence of the pain, and a pill, containing a grain of pure opium, was given every hour until relief was again experienced, which did not occur until next morning, when six or eight more of them had been taken. I now saw her again, and found appearances greatly altered. The quick respiration had altogether subsided—the pulse was slower—the pain nearly gone—the tenderness of abdomen, except at the right side, greatly abated, and she could turn a little in the bed. A purgative enema with about an ounce of turpentine and two quarts of oatmeal tea was now administered; pills, containing equal parts of aloes and extract of henbane, were directed to be given every second hour, and, if the pain recurred, a repetition of the opium pills. The bowels were abundantly moved after the lapse of a few hours, and there being a slight recurrence of pain afterwards the opium pills were resumed, but only at intervals of three or four hours, after which they were wholly discontinued.

I heard nothing of this patient until four or five days afterwards, when she drove on a car eight miles to see me. She was in every respect recovered, except in the tenderness of the right side, which still continued acute in the original site for about the breadth of the palm of the hand. This was evidently attributable to some organic mischief which was done to this part before any effective remedy had been resorted to for the inflammation. Repeated leeching—fomenting, and small doses of calomel and opium were ordered for her, and she eventually became quite well.

It must not be imagined, from all I have stated, that I believe purgatives to be at all times unadvisable in enteritis. These are perhaps occasions in which they are of use even in the early stage, but it is difficult to offer indications by which we shall recognize these occasions in practice. In the advance of the disease, when its force is considerably broken, and the bowels may be supposed

capable of acting without increasing or renewing the inflammation there must be an obvious advantage in getting rid of the contents of the bowels, and this may perhaps be then generally effected with safety, by means of mild purgatives, combined with henbane. If, however, there was no injurious distension present, and the inflammation was progressively declining, my disposition would be, to await a more perfect amendment before I would give even these. The only evils I should at all apprehend in these instances from confinement of the bowels are, irritation and uneasiness from restraining the natural actions and secretion, after once the decline of inflammation admits of their taking place; or at a later period, from actual distention by the intestinal contents.* These are certainly evils to be held in view and guarded against, though I believe by no means of that vital importance with which they have beeu, heretofore commonly invested. Without pretending to have satisfactorily solved the problem which I have yet ventured to discuss at such length, I shall merely recapitulate a few of the principal facts, as far as they appear to be such, and leave the inferences to the professsion. General experience testifies, that the strongest purgatives will not operate in the early stages of inflamed bowels unless large depletion by the lancet has been premised, that is, unless the violence of the inflammation has been in some measure subdued: while on the other hand, as soon as this has been accomplished, they commonly occur spontaneously, or with the assisance of the mildest purgatives

Notwithstanding the free operation of purgatives at an early tage of enteritis, the inflammation may proceed to a fatal termination, unless arrested by other remedies.

A purgative has been known to occasion inflammation of bowels, and when inflammation has been subdued by other remedies, it has brought on a recurrence of it.

Inflammation of the bowels may be perfectly subdued without any evacuation at all.

The bowels may even sometimes continue in a confined state for three or four days after the inflammation has subsided, without occasioning injurious distension.

* As mere distension of the intestines may occasion or keep up tenderness, it may happen after all inflammation and pain have been reduced, that the tenderness on pressure should continue. If there was great debility in such circumstances it might be a fatal mistake to withhold support or stimulants on the presumption that inflammation was still continuing. One's judgment should always be formed from the aggregate of symptoms.

PROBLEM II.

BY WILLIAM GRIFFIN, M. D.

HOW ARE NERVOUS AFFECTIONS DISTINGUISHABLE FROM INFLAMMATORY.

In treating of inflammation of the bowels and peritoneum, Dr. Abercrombie states as conclusions from facts, "that extensive and highly dangerous inflammation may exist in the intestinal canal without obstruction of the bowels, and it may go on to a fatal termination while these are in a natural state or easily regulated by mild medicines through the whole course of the disease."

"That extensive and fatal inflammation may be going on with every variety in the pulse; it may be frequent and small, it may be frequent and full, or it may be little above the natural standard through the whole course of the disease,"

"That extensive inflammation may go on without vomiting and without constant pain; the pain often occurring in paroxysms, and leaving long intervals of comparative ease."

"Keeping in view these sources of uncertainty," he says, "our chief reliance for the diagnosis of this important class of diseases must be on the tenderness of the abdomen. This symptom should be always watched with the most anxious care, *whatever may be the state of the bowels or of the pulse, or the actual complaint of pain,* and though the tenderness itself should be limited to a defined space of no great extent."

Whatever the experience of the profession may have determined with respect to the greater part of these conclusions before the appearance of Dr. Abercrombie's work, I believe the importance which should be attached to the presence or

absence of abdominal tenderness in cases of suspected inflammation has been always in accordance with the opinions of that distinguished physician; but Dr. Abercrombie was very well aware, when he gave such just weight to this symptom as a means of diagnosis, that in itself it was far from conclusive, and derived its value chiefly from the existence of many other corroborating signs. He states indeed distinctly in another part of his work, "that pain increased upon pressure does not appear to be a certain mark of inflammation in the bowels, for it occurred in Case XXIV., (related by him,) in which there was no inflammation; and in several other cases it was met with, probably, before inflammation had commenced. From various observations (he states) he is satisfied, that an intestine which has become rapidly distended is painful upon pressure; it is however, a kind of pain which, *by attention, can generally* be distinguished from the acute tenderness of peritonitis."

This point of diagnosis applies altogether to the discrimination of cases of ileus from those of inflammation, and even as such, is, it would appear, often of doubtful value; but in what light must we view it in reference to those frequent neuralgic affections, which, whether hysterical or the results of irritation of the spinal cord, are often established suddenly, with little preparatory disorder, and with no distension of the intestinal canal to account for the acute tenderness on pressure? No medical man is now ignorant of the fact, that the contents of both the thorax and abdomen as well as their parietes are subject to attacks of violent pain of a nervous or spasmodic character, yet with acute soreness to the touch, as in pure inflammations. These are so widely separated from the latter in their nature, and require such a very different, I might almost say opposite mode of treatmeut, that a correct diagnosis becomes a matter of still greater interest than in cases of ileus. There are few instances of ileus in which one effective bleeding might not be of service, while there are few of hysterical pseudo-peritonitis, or enteritis, or of similar affections arising from irritation of the cord, which might not be made worse, or indefinitely protracted by it—yet we find in our best elementary works on this subject a most perplexing indefiniteness, a diffuseness, of description, and a labouring at discrimination wholly unworthy of the present improved state of medical science, and unnecessary if any real

or essential difference of character could be pointed out. It is indeed, because there is an essential ageeement in all material points, that our attention is directed to the attitude, the expression of countenance, the manner of complaining, and even the temper of patients, although there must be sometimes very considerable differences as to all these in individuals similarly affected, and in any case, no slight degree of experience is required to form a proper estimate of their value. We are told that one affection is frequent, the other rare in comparison; that females are more liable to it than the other sex, and those of sedentary habits in towns, than those leading active lives in the country; all of which might be very useful information, so far as it tended to corroborate features of a more marked expression, but is essentially loose and vague, if considered by itself.—Again we are reminded of one or two truisms, that if a disease has lasted long and done no mischief, or if it has been aggrivated by an antiphlogistic plan of treatment, its character is not inflammatory. We may thus arrive at a correct diagnosis about the time the complaint ought to be cured and forgotten. But the climax of our difficulties on this subject is displayed in the admissions of experienced physicians, that their practice when in doubt, is to run the risk of erring on the safe side, and treat the disease as inflammatory;—perhaps a judicious plan enough under the circumstances, but certainly not so consolitary to the patient, nor as I have elsewhere remarked,* very creditable to medical science. It may be questioned too, whether a rule of the kind with young physicians would lead only to errors on the safe side. I have seen one valuable life lost, and others endangered by such practice, and sometimes the antiphlogistic treatment vigorously adopted, not indeed where doubts existed in the mind of the practitioner, but where the cases were altogether mistaken.

I am far from assuming to myself a perfect freedom from perplexity in all possible cases simulating inflammations of internal organs which may come before me, but with the facts I have stated impressed on my mind, I cannot but feel that the experienced physician is usually too well satisfied with that indefinable power of recognizing or identifying diseases almost unconsci-

* See Treatise on Functional Affections of the Spinal Cord.

ously acquired in the course of long practice. In reflecting on the melancholy steps by which it is attained, on the sad, though perhaps excuseable errors committed, he consoles himself with the conviction, that such experience only could gift him with a knowledge which neither books nor lectures had taught him; when he should rather feel, that the true reparation to his conscience, the real duty he had to perform in acquittance, was to prevent the occurrence of such mistakes with others, by analyzing the characters of those perplexing diseases, and endeavouring to trace the sources from which his late discrimination was derived. It is not enough for a practitioner in a difficult case to be assured he is himself capable of determining its nature; he should consider whether his knowledge or ability admits of being communicated to others, and when he believes it is, but not until then, he may also believe that he has acquired information of infinitely greater value to the public, than any thing it could have suffered from his early mistakes.

I have been led to indulge in these observations in consequence of the apparent inattention with which a suggestion with respect to the diagnosis of neuralgic affections, proposed by me some years since, was received by the profession: I then stated, that in any doubtful cases I believed if tenderness on pressure *at the portion of the spinal column corresponding with the disturbed organ* existed, it might be considered decidedly neuralgic, but if no such symptom was found, it was probably inflammatory.—The suggestion was then offered rather as a result of individual observation, which I was anxious should be tested by the universal experience of the profession, than with a view of claiming attention for an incontrovertible fact; but I have since so repeatedly derived ready assistance in simulated inflammation by assuming it as such, and have had the diagnosis so invariably borne out by the result. that I do not now hesitate to assert, it is almost the only single symptom upon which a young practitioner can rely without danger.

Without entering into the question of the nature of spinal irritation, which I have discussed at large in another place, I may be permitted to claim the reader's attention to two points connected with it, which must be regarded as physiological facts.—That the spinal cord, as the experiments of Le Gallois have shown, is composed of portions independent of one another in

their powers and functions, being centres from which the nervous actions of corresponding parts of the body emanate, and to which they tend; and, secondly, in conformance with the well known law, by which the pain and tenderness arising from disorder at the origin or trunk of a nerve, is referred to, or felt at its extremity, that affections of the spinal cord are not usually recognizable by pain at the part diseased, but at the terminations of the nerves in distant organs arising from it. From these facts there is one undeniable inference in determining the diagnosis of cases resembling inflammation,—that wherever spinal tenderness exists, we must at all events set down pain and tenderness (the two most important symptoms in assisting us to detect internal inflammation) as wholly valueless; inasmuch as, whether there be inflammation or not, these are not peculiarly the results of it, but may arise also from the tender state of the cord. I might perhaps go much farther, and assume, that where spinal tenderness exists, there also exists a state of the system scarcely compatible with acute inflammation.

If, then, pain and soreness on pressure, those supposed characteristics of inflammation, are of all others the most equivocal symptoms, belong as certainly to irritative as to inflammatory affections, and almost necessarily exist if there be acute spinal tenderness, what is to be our guide in deciding the diagnosis when called to a person suffering with violent pain of side, feverishness, difficulty of respiration, and soreness of the intercostal muscles; or with violent pain in the abdomen, occompanied by exquisite tenderness to the touch, and perhaps constipation and vomiting—*if we do not examine the spinal column*? We want to ascertain the simple fact in the first instance, of whether the pain is in the viscus supposed to be affected at all, or whether it be merely in the thoracic or abdominal parietes. And since the patient shrinks and complains on pressure in both cases, what, I ask again, is to be our guide, *if we do not examine the spinal column*? Are we to depend upon speculations on the attitude, expression of countenance, temper or manner of a patient, when we can at once lay a finger on the spine, and detect both the cause and nature of his complaint?

If in the former case we find acute tenderness of some of the dorsal vertebræ, or in the latter of some of the lumbar, I do not hesitate to say, the one is not pleuritis, nor the other ente-

ritis, nor will either bear large depletion with impunity. The pain and tenderness are merely referred to the extremities of the spinal nerves, ramified through the intercostal or abdominal muscles, from the affection of the corresponding portion of the spinal column, and indicate nothing whatsoever of the state of the viscera internal to them, which usually excites so much alarm. If these are facts, and I believe few will deny them to be so on examining for themselves, is it not absolutely leading the young practitioner into those mistakes which we so much deprecate, to associate pain and tenderness on pressure so exclusively in his imagination with inflammatory disease? Is it not strange too, that cases are every day published in our periodicals with dissertations on the difficulty of their diagnosis, without the slightest allusion to the spinal cord, the state of which, in every case of presumed inflammation, our present knowledge of physiology must shew us the necessity of ascertaining. One would suppose where the obscurity of the diagnosis is often so undeniable, that any adventitious light which could be brought to bear upon it would be sought for with avidity, but this, every day's experience assures us, is very far from the reality. As illustration is very generally more impressive than argument, I shall offer two or three cases to the consideration of the reader.

A young woman aged 25 years, was attacked with pain in the bowels at night after a feeling of chilliness. She took some essence of peppermint and went to bed, but the pain gradually increased, and at 2 o'clock in the morning she took twenty drops of laudanum. At 7 o'clock, the pain still continuing, she took castor oil with ten or fifteen more drops of laudanum by the directions of an apothecary, and soon after ten drops were repeated in a saline draught. I saw her at one o'clock, and found her writhing with pain, chiefly round the umbilicus and to the right side. It became more violent by fits like colic, though never entirely subsiding, and during the intervals of comparative relief she sometimes threw her arms about restlessly, sometimes lay, as if insensible, with the eyes turned up and the lids half open. Her complaints were low, scarcely audible, her respiration painful when deep; turning from side to side increased her pain, although when the paroxysm occurred she turned sometimes on her face. There was excessive tenderness of the abdomen; the least

pressure making her scream.—She had been constantly vomiting for the last few hours. The castor oil had operated once scantily, her pulse was but little quickened, and there was no heat of skin.

Here was a case of constant pain in the abdomen chiefly about the umbilical region, liable to severe exacerbations, attended by exquisite tenderness on pressure, vomiting and constipation, and continuing for twenty-four hours. I am convinced that almost any young physician would have felt great difficulty, indeed almost an impossibility of determining from a consideration of the symptoms, that the complaint *was not inflammatory*, and I believe that the great majority of either young or experienced ones would at once infer the existence of inflammation, and bleed. I say so from having witnessed it, and from having early in my own professional life always prescribed in such cases with timidity, as if I felt that all consideration of the symptoms led to little better than conjecture. I had now, however, new means of diagnosis in the state of the spinal column, on examining which my mind was set at rest. As soon as I pressed on the upper lumbar vertebra the girl started violently, caught my hand, and complained that I had hurt her dreadfully: the pressure, she said, had increased the abdominal pain; she had never had any hysterical attack in her life. I ordered the abdomen to be fomented freely, and gave her five grains of calomel with half a grain of opium, directing five grains of aloes and five of extract of henbane, to be taken every two hours. The calomel and the first dose of the aloetic pills remained on the stomach, but the succeeding doses were rejected; a purgative draught was also thrown off; she then got a purgative enema which was repeated in an hour, but both passed off without any appearance of fæces. The vomiting, pain and tenderness of abdomen continued very severely throughout the evening. At night she got a turpentine enema and was placed in a hot bath which gave some relief and procured a scanty evacuation. The pain, however, soon recurred, and she was ordered a grain of opium, to be repeated every hour until it should subside. The bowels were freely moved soon after taking the first dose, but she did not experience any considerable relief until she had taken the third, after which she fell into a sound sleep, and in the morning was in every respect improved. The pain had altogether subsided, and the exquisite tenderness

was now felt in the right epigastric region only. She had threat enings of a return of the attack in the course of the day, but it was readily subdued by a repetition of the opiate. On the following morning there was scarcely any pain or tenderness, and if the complaint had been inflammatory, I would now have left the patient's bowels perfectly at rest. But believing it to be an affection of the spinal cord, arising from disorder of the digestive functions, I ordered another dose of castor oil, which operated freely, and was followed by no recurrence of the attack.

Mrs.——, aged 30 years, of a full habit, complained of pain in the right iliac region, which gradually increased until the whole of the abdomen became sore to the touch ; on the third day she took castor oil, which operated freely, but gave no relief. I saw her on the fifth, when she complained of being much worse, and directed her to go to bed, to have the bowels fomented, and to take pills of aloes and henbane every second hour until relief was obtained. In the morning I received a letter, stating, that she had been in agony all night, was much swelled, had not had her bowels moved for three days, and was now feverish and throwing every thing off her stomach. I became excessively alarmed, and regretted I had not bled her on the night before. On arriving at her residence, I found her complaining of violent pain all over the abdomen, but most acutely in the right iliac region, and almost down to the pubes at that side. There was some fulness of the bowels, and the greatest tenderness on pressure. She could not bear the slightest touch in the iliac region, and one spot she described as exquisitely sore. It was on this account, perhaps, that she bore the weight of my hand laid flat, and pressed very gradually, better than pressure with the points of the fingers, as in the former case the pressure on the most painful spot was not so direct. Her pulse was ninety-six, her skin feverish, and tongue white ; she had nausea and occasional vomiting, and could not turn or move in the bed without considerably increasing her sufferings. The only symptom that could lead me for a moment to doubt that I had here a case of very serious inflammation to deal with, was the expression of the countenance, which did not betray the deep anxiety and distress I have usually seen in the inflammation of vital organs,

although her own description of her suffering was sufficiently alarming.

The treatment of the case with decision and confidence now depended altogether on the examination of the spine. If I found a state of increased sensibility and excitement of that part of the cord which gave origin to the lumbar nerves, the pain and tenderness of abdomen would be accounted for, and I need not infer a more alarming cause for them. If, on the other hand there was no spinal tenderness, the existence of acute inflammation was almost certain, and the most active treatment was demanded. On pressing the spinal column as soon as I reached the lumbar vertebræ, she started and screamed aloud. She also felt excessive pain on my touching the sacrum, or making pressure behind the trochanter, or at the front of the hip joint in the groin. I therefore unhesitatingly concluded, it was one of those nervous affections simulating inflammation, in which the progress of the case fully bore me out. A few leeches were applied to the most painful part of the abdomen, and a grain of opium was directed every second or third hour until there was some abatement of pain; she was also directed as soon as the vomiting abated to resume the pills of aloes and henbane, and take a dose of castor oil. She got considerable relief, but passed a restless night. In the morning she took the aperient medicine, which operated freely, and now had pain occasionally only, and at long intervals; the vomiting had ceased, but the soreness of the abdomen was still exquisite in the right ileum and groin, and she screamed when I pressed on the vertebra or immediately behind the trochanter. Fomentations were now directed: belladonna plasters were afterwards laid over the painful parts, and the opium pills were given every fifth or sixth hour. She spent the day well, but the pain recurred in the night very severely, and was relieved by the anodynes. Next morning the bowels were again moved, but the soreness on pressure or on moving much in the bed still continued. The case went on in this way for several days, abating very gradually, but the soreness of the abdomen and of the hip joint was not perfectly removed until she was for some days driving out in her carriage.

I attended this lady afterwards for an attack of acute bronchitis, which was of a similar character in its advanced stages.—The cough, oppression, and expectoration, were evidently the

result of nervous irritation, and required tonics rather than the continuance of any antiphlogistic treatment. She told me that in a former illness, which she called inflammation of the lungs, the doctor had bled and, I believe, blistered her more than once, that she was going on from bad to worse with incessant cough and debility, until at last, in defiance of all advice, she took a glass of pure wine, which cured her like a charm. I mention this, not that one can infer much from such loose statements, but because it gives some idea of the irritability of her nervous system.

Mrs.———, of a delicate, nervous habit, after a natural labour, had internal hæmorrhage, with pallid countenance, cold clammy sweats, chilliness, and an almost imperceptible pulse at 159; the hæmorrhage ceased on the extraction of several clots but the debility continued to an alarming degree for several hours. She had large doses of laudanum with dilute sulphuric acid through the night, which seemed to relieve her much; but in the morning the debility was still great, the pulse weak at 130, the features sunk, and the respiration much hurried. On the following evening she had a severe rigor, and was soon after attacked with pain and tenderness in the uterine region, excessive pain in the head, brow, and eyes, with sickness of stomach and vomiting; there were thirst and heat of skin, and the pulse became rather hard at 140; the tenderness on pressing over the uterus was considerable, the pain constant, the lochia diminished. On examining the spine, there was found acute tenderness of the lumbar vertebræ, upon which it was assumed to be a case of nervous irritation and not inflammation. A dose of calomel with extract of henbane was given, fomentations were applied to the lower part of the abdomen, and she got diaphoretics at intervals. Under this simple treatment the symptoms declined, and on the succeeding night she felt quite relieved; the lochia and secretion of milk becoming abundant, and the pulse soft at 125. After the lapse of some days she was up and well.

Mrs. M———, aged 25 years, had very severe flooding in her lying-in. On the fifth day after, she was seized with pains in the abdomen, attended by excessive soreness to the touch, and ver. She got castor oil on the sixth day, which operated twice without giving much relief; in the evening she had a slight rigor

and felt very weak; the milk had left her from the first moment of the attack, and the lochia had nearly ceased. I saw her for the first time, on the night of the seventh day from her delivery, and found her lying on her back, with her knees up, complaining much, and incapable of the least motion, or turning to one side or the other without the greatest torture. The pain was constant, but was increased to an excessive degree at intervals; coughing was very distressing to her, and the least pressure on any part of the abdomen made her scream; slow, steady, cautious pressure with the flat of the hand was unbearable, but she could stretch down her limbs without increasing her sufferings; her countenance, which was pale from the flooding, had an expression of distress, and the brows were knit as with pain; the skin was warm, the tongue moist and white, and the pulse weak at 112; there was excessive tenderness of all the lumbar vertebræ. The abdomen was fomented, and she got two grains of calomel and a grain of opium every third hour. After the second dose the pain abated, and she got a good deal of sleep, but in the morning the symptoms returned, and the pills were resumed with the same good effect as before. The tenderness of abdomen was now rather less, though the bowels appeared more full, not having been moved for the last forty-eight hours; she could turn from side to side with less suffering, the pulse still 112, and the skin warm and moist; she took castor oil, and afterwards pills of aloes and extract of henbane every second hour, which operated freely; she passed a tolerable night and seemed easier, but got the pain more violently than ever on the following morning, when it was relieved by 40 drops of laudanum; it continued in the intervals of these paroxysms, but not severely. In the evening the paroxysm recurred, and was again relieved by the laudanum. Notwithstanding these recurrences she felt herself on the whole much better, could turn from side to side with more freedom, and wished for nourishment; she complained of some headach. She was now directed to take three grains of sulphate of quinine three times a day, and to repeat the laudanum if seized with the pain; I also allowed her to get a little chicken broth or beef tea. She passed a good night, and although the pain recurred at intervals on the following morning, it was much less severe; she threw up some aloetic and henbane pills which she had taken to free the bowels; the abdomen, though still pain-

ful on pressure, was much less tender; the soreness of the spine was nearly gone, and her milk was returning. As her bowels had not been moved for the last thirty-six hours she got some castor oil, and the quinine and broth were continued; she complained much of the headach.

The pain recurred again violently in the night, soon after which the flooding returned for a short time, and then ceased; the pain was relieved by a grain of opium; felt much better through the day, and ate a little meat without permission; the soreness of abdomen almost gone, and none whatsoever of the spine; her headach was better; she continued her quinine and nourishment, and was well in a few days.*

I might multiply cases of this nature to an extent that would be tiresome, without making the point much clearer. Perhaps it may be said, that an experienced eye would have detected some anomalous symptoms in all these, which would have led to doubts of their inflammatory nature. I am not disposed to deny that much may be inferred in such attacks from the suddenness with which violent symptoms supervene, the absence of deep distress and anxiety of countenance, and above all from a freedom in the

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* The above case at the first moment I was called to it, so perfectly answered the descriptions given of simple peritonitis, (see Abercrombie on the Viscera, p. 151,) and so truly resembled the cases of that disease which had fallen within my own experience, that I watched its progress with much anxiety. I was indeed somewhat distrustful of the diagnosis, for although the extreme tendern ss of spine would fully account for the tenderness of abdomen, the great torture experienced on the least motion from side to side, was a symptom more characteristic of true peritonitis than of a neuralgic affection; the progress of the complaint, however, fully satisfied me of its nature. I believe there is no practical physician will assert that a case of acute peritonitis would have been arrested by the treatment adopted, which was in fact little else than palliative. It was only on the first day of my attendance that the opium, combined with calomel, was given with any regularity five grains having been taken within twenty-four hours; for although the pain recurred with equal violence subsequently, she had seldom occasion to take an opiate more than once or twice in the day. I may, however, point out one symptom in which the case differed from peritonitis—the ease and freedom with which she could stretch down her limbs. In spinal affections the least motion of the spine, especially turning or twisting motion, will sometimes increase the abdominal pain, but it scarcely ever occasions difficulty or uneasiness in the motions of the lower limbs.

movements of the lower extremities, unusual in acute abdominal inflammations. There was indeed at least one of these discrepancies observable in a greater or lesser degree in each of the cases detailed: but how are young practitioners to form a diagnosis on such grounds. People suffer similar degrees and kinds of pain with very dissimilar degrees of fortitude, and at all events any reasoning on such signs must be founded on comparisons, which, to be worth any thing, would imply an experience no young practitioner can be supposed to possess.

In all acute inflammations of vital organs, I believe that no spinal tenderness will be found, except where it existed previous to the supervention of the attack,* or where the spinal cord itself happens to be the seat of such inflammation. In all neuralgic affections on the contrary, tenderness of some portion of the spinal column, *usually that corresponding to the affected organ* may be detected, except in some rare cases in which it seems probable the ganglionic nerves alone are concerned. As these cases must still present a difficulty in their diagnosis, we must rest contented with those general characteristics, which, however vague or liable to lead us into error, are all we have to guide us, and all we have hitherto had to determine our opinion in that large class of neuralgic affections, for the detection of which I have here been offering a new, and I believe less doubtful sign.

The observations offered respecting the diagnosis of cases resembling acute inflammations, apply with almost equal truth to those resembling chronic diseases; I mean those pains affecting the chest, attended by cough and perhaps oppression, leading to apprehensions of phthisis; affecting the side below the false ribs and suggesting affections of the liver or bowels; or affecting the pubic region and simulating disease of the womb or ovaries. It is, however, wholly unnecessary to extend this paper to an unreasonable length on the subject of those nervous affections; the nature and diagnosis of which I have discussed so fully in another place. I need only remark, that deducing inferences from acute or chronic pain apparently affecting any viscus, without examining the state of the spinal cord,

† Some writers on spinal irritation state that they have found spinal tenderness with inflammation of liver.

seems to me little less absurd than omitting to examine the state of the hip joint in those painful affections of the knee, the nature of which is not immediately obvious. In either case the disease may not exist at all in the part apparently affected, and the real source of the pain can only be ascertained with certainty by an examination of the origin of the nerves distributed to it, or of any organ with which the part affected may be supposed to sympathize.

PROBLEM III.

BY WILLIAM GRIFFIN, M. D.

WHAT IS THE DIAGNOSIS OF ABDOMINAL INFLAMMATIONS.

The following cases are intended merely as new and striking illustrations of a fact, to which I have in a former paper endeavoured to direct the attention of the profession, viz, that inflammatory, or other affections of the spinal cord, or of its nerves, at their origin, more frequently simulate abdominal and thoracic inflammations, and are more frequently mistaken and maltreated, than is at all imagined. As I know of scarcely any cases in which the diagnosis is so difficult, where the practitioner is not fully informed on the subject, or a mistake so likely to be attended with very deplorable consequences, I shall, I hope, be excused for returning once more to their consideration.

There are three effects very common to inflammation, or disorder in the spinal cord, or at the trunks of its nerves. First, superficial tenderness, more or less exquisite, and either limited to the integument immediately over or about the affected portion of the cord, or extending thence to the front of the abdomen or thorax, in the direction of the spinal nerves, or occupying the whole surface of those parts of the body, which are below the portion of the cord affected. Secondly, pain either close to the affected portion of the cord, or at the extremities of the nerves which have their origin there, or in the ganglionic nerves supplying the viscera, which have connexion with that portion of the cord. Thirdly, loss of power evinced in partial or complete palsy of the parts or organs, to which the affected nerves are distributed. Their effects often occur simultaneously, but any one of them may also occur independently of the existence of the others, offering very

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strong evidence, that the sensibility of the surface or skin, and that which exists in internal organs, is dependent upon nerves, which though sentient, are as distinct from one another, as they are from those on which the power of motion depends. Keeping these ordinary effects of disorder of the spinal cord in view, it must appear obvious, when we detect soreness or tenderness on pressure in the region of the liver or spleen, or in the lower abdominal viscera, how important and essential it is for us to ascertain whether the soreness be superficial or deep seated, which we can always do by careful examination. Again, when pain is complained of in the region of the liver, or middle, or lower parts of the abdomen, how necessary is it to ascertain, whether, as in the case of soreness, it be superficial, and, if deep seated, whether it be merely an affection of the nerves of the part, and probably connected with some affection of the adjoining portion of the spinal cord, or whether the internal organ itself be in a state of acute or chronic inflammation. Finally, if there be oppression in breathing; whether it arises from deficient action in the respiratory nerves, and, as a consequence, imperfect action of the respiratory muscles, or the imperfect performance of the process of oxygenation of the blood in the lungs, or from actual inflammation, or organic disease of the mucous membrane, or parenchyma of these organs. Or if there be obstinate constipation of bowels, whether it depends on spasm or enteric inflammation; or whether purely on deficient power, or partial palsy of the muscular fibres of the intestines. I know of no case which so often deceives inexperienced or uninformed practitioners as obstinate constipation of bowels from paralysis of the nerves, when connected, as it frequently is, with abdominal pain and exquisite soreness on pressure. The following case appears to me to be highly illustrative of the fact.

The captain of a merchant vessel, a hale, hardy man, aged about 45 years, while at sea, was attacked with pain in the back and belly, and obstinate constipation. Having no medical advice, he took successive doses of purgatives, until he exhausted every medicine of that description in his medicine chest without avail. At the end of nine or ten days the violent pain and constipation yet continuing, he reached the river Shannon, when an apothecary came on board, and gave him large doses of croton oil, which eventually purged him severely, *but without any relief to the pain.* The bowels became again constipated, and after a few days con-

tinuing still in extreme suffering, a physician was sent for, who bled him largely, and directed fresh purgatives, both by the stomach and by enemata. The bleeding gave considerable relief, but the bowels continuing confined, and the pain returning on the following evening in a violent degree, I was sent for to consult with the physician in attendance. I found the patient complaining of great pain in the abdomen and round the loins, with difficulty of passing water and obstinate constipation. The entire surface of the abdomen was very tender to the touch, and in the pubic region excessively so; there was also over nearly the whole trunk of the body an acute soreness of the skin, especially about the hips and lower extremities, which made it painful to turn himself in the bed, or to permit any one to assist him in doing so; there was great tenderness on pressure along the whole spinal column, but he complained of the pain only in the lumbar portion, and in the corresponding parts of the abdomen in front; his skin was hot, his tongue foul: his pulse small and rather feeble, at 128; he had been bled to the amount of more than a quart the night before, and was continuing the purgatives and enemata.

If, in this case, the general soreness of surface, of which the abdominal tenderness formed but a part, and the general soreness of the spinal column were left out of consideration, which they necessarily would have been by any practitioner who did not look for them, I know of no symptom which could have led one to doubt the disease was any other than peritoneal inflammation running into enteritis. The whole appearances were, indeed, so characteristic of one or both of these affections, that I had myself some hesitation on the subject, although my belief was, that the disease from the first was some inflammatory affection of the spinal cord, or its membranes, occasioning pain in the back and viscera, and accompanied by that superficial soreness which attends almost all severe spinal affections, as well as by the constipation, which is so common a result of deficient power in the muscular fibre, whose motor nerves are in an inflamed or disordered state. The physician in attendance agreed to suspend the exhibition of some croton oil which he was about to give, on my explaining my views of the case, and to substitute repeated doses of opium combined with calomel. To satisfy his apprehensions however, two dozen leeches were applied to the public region instead of to the lumber vertebræ, or sacrum, to which I felt they

ought to be applied, and a simple purgative enema was administered. The leeches bled freely through a great part of the night, and he took twelve pills within the same number of hours, containing two grains of calomel, and one grain and a half of opium in each, without relief to the pain. He had no head-ache from this quantity of opium but his stomach was somewhat sick, the enema had come away without any admixture of fæces, the tenderness of abdomen, and other parts of the surface, the temperature of skin, and the state of the tongue and pulse continued as before. Pills of crude opium, (one grain in each) every hour, were now substituted for those with calomel and opium, the enema was repeated, and with much reluctance I consented to the anxious desire of my medical colleague to apply a blister to the abdomen, from which I augured little good. At our evening visit, the blister had risen well, but the symptoms remained unaltered; on the following morning we found he had passed a restless, painful night; the pain of the abdomen had abated, but the pain of the back was as distressing as before, and elicited continual complaints from him. He had now taken since first I saw him, eighteen grains of opium within the first twelve hours, and nearly twenty-four grains within the last twenty-four hours, without any considerable relief to the pain, without any sleep, or even drowsiness, and without any head-ache. I had now some difficulty in inducing my colleague to continue the opium treatment, as it was apparently doing no good, and purgatives seemed so much more strongly indicated. On stating to him, however, my perfect confidence in the influence of the opium in controlling inflammatory action, (suppose it existed) in the intestines, and that the latter would most readily act when once the inflammation was subdued, he consented to the continuation of the treatment a little longer. As there was a possibility that the opium administered might not have been of good quality, we now directed a draught containing a grain of acetate of morphia, with a drachm of tincture of henbane to be given, and a similar dose to be administered in two or three hours, if there was neither abatement of pain nor sleep. To our gratification, several hours' quiet sleep followed from the draught, after which he became restless and uneasy; but on getting the second draught, slept again quietly for the entire night. In the morning we found him for the first time free from pain, his pulse slower, and feverishness less, and ordered him an

ounce of castor oil, to be followed in a few hours by a purgative enema. My confident anticipation that, when once the pain and inflammatory symptoms were subdued, mild medicine would be found to operate, where before the strongest doses often repeated had failed, was now fulfilled; the castor oil and enema purged him freely: he got a repetition of his anodyne draught afterwards and slept soundly a second night. On the succeeding morning his pulse was fallen to 100, his tongue was cleaner, and his urine less turbid. He also for the first time evinced some disposition for food. His abdomen was still excessively tender, although he had no pain. We directed a repetition of the castor oil, and after its operation half the anodyne draught to be given at bed time.

It would probably be argued by many medical men, that in assuming this to be a case of inflammatory affection of the spinal cord or its membranes, I was doing so quite gratuitously, that it was from the commencement a case of colic or ileus terminating in peritoneal or enteric inflammation, and that the treatment employed, which eventually proved successful, was well known to be such as would generally give relief in like cases after large bloodletting. I should, perhaps, not insist so positively on the view I took of the disease, if the sequel had not amply borne me out.

On our next visit, November 15th, we found the pulse had fallen to 85, the tongue was yet cleaner, his bowels had been well moved, and he passed his water freely. The spine however was still tender, and the soreness of the abdomen as before; he complained also of soreness and weakness of the left arm, which was now found paralytic, and extremely sore to the touch. It lay helpless by his side, he could neither lift nor move it, but could flex and extend the fingers; his appetite was good, his skin cool, and his pulse natural. A strong stimulating liniment was directed to be rubbed on the spine night and morning, and a little meat and wine were allowed. He went on doing tolerably well for two or three days, gaining strength gradually, the arm, however, still continuing helpless. On the 20th, he got a change for the worse, complained of weakness, especially in the right arm, which had not been before affected; his pulse was quicker, his appetite impaired, and the urine became again high coloured, with a copious pink deposit on cooling. We discovered that he had been for the last day or two fretting much about his ship and private affairs, and that he had on the previous night eaten and drank to an ex-

travagant amount. On the following day, November 21st, his right arm was also paralyzed, the power of moving the fingers alone remaining; his pulse was 120; his skin warm and disposed to perspire. There was no feverish heat however, no pain, no nausea but his appetite was gone, and his bowels confined. It is unnecessary for the object of this paper to enter minutely into the detail of a very protracted case; it will be sufficient to mention, that each of the lower extremities became successively paralyzed, that afterwards his neck became affected, and he spoke in hoarse whispers, and subsequently complained of giddiness, or reeling in the head, with head-ache. The treatment consisted in successive blisters along the whole course of the spine; moderate purging, and the hydriodate of potass three times in the day. When all feverishness subsided, when the pulse became natural, and his appetite returned, bitters were directed, with a twelfth of a grain of strychnine every sixth hour. Under this treatment the power of the lower limbs was gradually restored, and the arms recovered sufficient power to admit of his elevating them a little. The fingers of both hands however are flexed into the palm, and from the weakness of the extensor muscles he is unable to extend them. He is since walking about with his arms in slings, and there seems to be a progressive, though slow amendment, which leads me to anticipate their eventual recovery.

I shall now state another case equally convincing. A gentleman aged forty years, of spare habit, but hardy constitution, was affected with a severe feverish attack, consequent to getting wet and chilled by a long journey. He had intense head-ache, sickness of stomach with distressing retching, and disposition to constipation of bowels for several days. A dozen leeches were applied twice to his head, and bled profusely; his hair was cut, cold lotions were constantly applied to the vertex and forehead, and he was repeatedly and freely purged. He never had much heat of skin; the pulse was quick, but neither full nor hard; it was, in fact rather a weak pulse. In a fortnight from the commencement of his illness, during the first week of which he had no medical attendance, he was evidently better, his pulse slower, his tongue a little cleaner, the sickness of stomach less, and his skin quite cool. He still complained however excessively of his head; said he felt a queer, indescribable feeling, as if it was too large for his body; complained of noises; and though the nausea

had abated, had not the least desire for food. He required very strong purgatives to move his bowels ; and I observed that whenever he got one, he was always worse while its action continued, which was usually for eighteen or twenty hours ; the unpleasant feeling of the head and the noise and sickness of stomach becoming more distressing, although he commonly felt better the day after. He appeared to me to be of a very nervous temperament, and informed me that in his best health, when he took a purgative, he was always made ill for the day. As he had at present a constant unpleasant feeling like indigestion at the stomach, and generally a sense of fulness about the bowels when they had not been recently moved, he was himself anxious for the purgatives, much as they disagreed with him. After having taken an active one at this period of his illness which operated severely, he passed a tolerable night; but about the middle of the following day he was seized suddenly with the most violent pain in the abdomen and back which I ever witnessed. The pain was constant and worst about the navel, but it was most agonizing in the lumbar region ; his skin was cool, his pulse rapid and weak, his countenance clammy with moisture and expressive of the utmost suffering; the abdomen was sore to the touch, especially when the pain was greatest, and the stomach was sick, although there was no retching. It would be difficult in fact, to distinguish the case from one of those severe attacks of enteritis in the onset of which it is said, there is such depression of vital power that the pulse is small and feeble, but acquires strength and volume after blood-letting. I should be sorry to recommend a practitioner to bleed in such circumstances in any case, although, until lately, in most treatises on inflammation of bowels we are told not to be intimidated by the state of the pulse. Opium is the great and only defensible remedy, whenever the least doubt can be entertained, either as to the nature of the attack or the strength of the patient. If the case be pure inflammation, it is as capable of subduing it as the abstraction of blood, while, if it be not inflammation, or though a case of inflammation, if the powers of life be much sunk by previous illness, the patient is irrecoverably lost by a large bleeding. I have seen a sad instance of this even where there was no mistake as to the nature of the case. In the instance I am narrating, thirty drops of laudanum and a grain of opium were given, and afterwards, a large quantity of spirits of camphor and lau-

danum was rubbed into the back and belly; large poultices of bran, moistened with boiling water, were applied, enclosed in flannels, to both situations. A grain of opium was subsequently given every half hour, until he expressed a sense of relief from the pain, which did not occur until he had taken in all about six grains of opium. After the pain had entirely subsided, drowsiness and a sense of sinking came on, with sickness of stomach and occasional retching; his pulse was feeble and thread-like; his skin cold and clammy, and large drops of cold perspiration bedewed his forehead. He had a great desire to sleep, but as soon as he dropped off, the sickness of stomach came on, and he was compelled to start up from the pillow and call for the basin. I now directed him to get burned brandy in teaspoonsful at short intervals until the sinking and retching were relieved, which took place in two or three hours, when he got quiet sleep until morning. He then appeared much better in every respect, but complained much of soreness near the navel, where the severest pain had been. The stomach, too, was yet unsettled, the head uneasy in the manner it had been all through, and he had no appetite. He also complained of some difficulty in passing water, a common consequence of large doses of opium. For this, sweet spirit of nitre was ordered, and, as nourishment, some chicken broth and arrow-root. He went on improving for two or three days, and then became affected with pain and soreness of the right side. The tenderness was most acute to the touch, immediately over the situation of the gall-bladder, and extended along round the margin of the ribs to the back; no other part of the abdomen was now tender but this, and there was no tenderness of the spine My patient could hardly be persuaded by any argument, that he had not got a liver complaint, and if the situation of the chief pain and soreness, the constant nausea, and sometimes retching, which he had from the commencement, and the foulness of the tongue which he had never lost were alone considered, few medical men would have disagreed with him. I had considerable hesitation in coming to a conclusion myself on the subject; but as I was exceedingly unwilling to weaken or torment him unnecessarily by leeching, blistering, or mercury, I investigated the case most minutely. On endeavouring to ascertain accurately the boundaries of the soreness, I found it extended below the margin of the ribs, down to the spine of the ileum, which was not likely

to occur in chronic inflammation of the liver, unless where there was great enlargement; I found also, that although he actually screamed, or started, when I touched his side ever so slightly, I could by pushing the fingers of both my hands towards the liver, one from the right side and the other from the left, so as to keep the tender muscles or integuments outside them, make as great pressure as I pleased on that organ without giving the slightest pain. I satisfied myself, in fact, that the pain and soreness were altogether in the integuments, and instead of annoying my patient with painful or weakening remedies, I ordered tonics, and the application of a belladonna plaster to the painful part. In two or three days, he was much stronger and free from pain. After a lapse of some days more, however, a new complaint presented itself, pain and soreness in the region of the bladder, along the course of both spermatic cords, and in the testicles, with a feeling of want of sufficient power in expelling the urine, and some sensation of heat. A belladonna plaster was now placed over the pubes, extending to each groin, after the application of ten or a dozen leeches, which I was induced to apply in that situation as there was no obvious tenderness of spine. The heat and tenderness were abated by the bleeding, but the other symptoms continued, and the weight of the testicles kept up such pain along the spermatic cords, that he was unable to walk across the room without a suspensory bandage. He had for some time been taking a grain and a half of sulphate of quinine with three or four grains of extract of hyosciamus three times a day, and his appetite and strength were very much restored. As the pain and soreness already described continued to a very troublesome degree, I again examined his spine, and found a small spot exceedingly tender, near the upper part of the sacrum; pressure on which brought on the pain in front. Being perfectly satisfied that the mischief lay in this situation, I immediately directed the application of a narrow blister to the sacrum, and on the following morning found my patient relieved from all pain and uneasiness. The result, appeared to him almost magical, as he could hardly comprehend that the source of all his annoyance should exist in a distant part where he complained of no pain! The only unpleasant feeling that remained, was a slight consciousness of inability to expel his water with the ordinary force, which gradually subsided.

Experienced practitioners will probably think, that I have been unnecessarily minute in the details of these cases, but they cannot claim the attention of the profession too strongly, when the diagnosis of abdominal inflammations is often so difficult, and when tenderness or soreness on pressure, to which Abercrombie and other men of celebrity attached the highest importance as a diagnostic symptom is known to exist, in an exquisite degree, in affections of the spinal cord or it membranes. When it would, in fact, appear, that, considered by itself it is one of the most fallacious, instead of being one of the most certain signs of inflammation.

In determining the diagnosis of abdominal inflammation, where both pain and tenderness on pressure exists, we should always endeavour to ascertain:

First.—Whether there be any pain or tenderness on pressure in the corresponding portion of the spinal column; because, if there be, although it may not absolutely decide whether inflammation be present or not, it is quite sufficient to account for both the pain and tenderness, without assuming the existence of any inflammation.

Secondly.—Whether if there be no spinal tenderness or pain, the soreness of the abdomen be superficial or deep-seated, which may be ascertained with tolerable certainty in all cases, by an examination directed to that end. And whether, if both superficial and deep-seated, as it usually is in peritoneal inflammation, gentle, steady pressure with the flat of the hand can be easier borne, than with the points of the fingers. In pain and soreness from affection of the spinal nerves it commonly can be so borne, while in peritonitis every kind of pressure, and even the weight of the bed-clothes, is very distressing.

Thirdly.—Whether the boundaries of the pain or soreness extend beyond what the suspected inflammation could produce? Thus, if inflammation of the liver be suspected, and we find the soreness extending to the ileum or groin, or to the opposite side of the abdomen to which the liver does not extend, it is obvious the soreness cannot be attributable to mere disease of that organ. Again, if the whole abdomen be tender to the touch in a case otherwise closely resembling peritonitis, and we find the tenderness is not confined to the abdomen, but extends over the hips and lower extremities, it is obvious, we can attach

no importance to the abdominal soreness as a sign of inflammation.

Finally.—It should be recollected that constipation may depend on mere loss of power in the intestinal nerves, as well as on spasm, obstruction, or inflammation, since the treatment in each case must necessarily be modified, or directed by the supposed cause of this symtom.

PROBLEM IV.

BY WILLIAM GRIFFIN, M. D.

IN THOSE DISORDERS WHICH HAVE GONE UNDER THE NAME OF SPINAL IRRITATION, IS THERE REALLY ANY AFFECTION OF THE SPINAL CORD OR ITS MEMBRANES.

The foregoing question is one which has been for some time in the course of slow but steady investigation ; and I should not now, after the contributions which I have already given to the public on the subject, feel it necessary to bring it forward again, if it met with the same consideration from the reviewers which it has done from a very talented portion of the profession. I have only within these few days seen in one of the best conducted periodicals of the day, a notice of Dr. Marshalls new work on spinal irritation, which contains a very detailed enquiry into the proofs upon which the existence of such a complaint is assumed. The writer of the article is evidently opposed to the new doctrine respecting spinal irritation, but speciously as he reasons on the subject, his strongest objections seem to apply more to the irrelevance or inaptness of many of Dr. Marshall's cases than to the principle he advocates. I cannot indeed conceive any thing so unphilosophical as the habit, if I may so term it, with medical critics, of slighting all evidence and all legitimate induction, with respect to any new proposition in medical science, because it has been strained beyond its bearing by some over-sanguine inquirer—except the still less excusable one, of arraigning it at the bar of public opinion without bringing all the proofs which may make in its favour, as well those which make against it, to judgment. A question of the kind is not to be decided by the merits of a few cases, nor by the reasoning of a single advocate. Whenever it is agitated at all, the whole of the known facts which make for or against the existence of any affection of the spinal cord or membranes, in the complaint referred to, should be deliberately weighed, one against

another, and then if the Scotch verdict, "not proven," is returned, we should be, at the least, satisfied that it did not follow from any unfair prejudice. Without any reference to Dr. Marshall's work, I shall now just run over the general objections which the reviewer makes to the doctrine of spinal irritation, and afterwards bring forward the facts and arguments in support of it, from which the profession are at liberty to draw their own inferences.

In the first place, he states, "not only is the nature of what is thought to be the primary pathological condition of the affection designated by the term spinal irritation, shadowy and indistinct, but the texture or part in which it resides is unknown." Here are two very positive assertions made, and certainly, if we had no stronger reasons for inferring the seat of spinal irritation than we have for inferring its nature, very correct ones. We really know nothing at all of its nature, or the pathological condition with which it is connected, and I fear we are likely to remain a long time in our ignorance. The morbid anatomy of the disease never comes before us, and if it did, even frequently, I doubt much that we should detect any diseased alteration which we could unhesitatingly connect with the symptoms. But is it absolutely necessary to our knowledge of the site of a disease, or to its successful treatment, that we should be acquainted with its precise nature, and be able to tell whether it arose from "*neuralgia,** *or congestion, or inflammation, or something different from all these.*" What is the pathological condition of the brain in the several forms of mania, or in epilepsy, or in chorea? Are we to look for the morbid action and the proximate cause of the mental aberrations or other phenomena of these affections in other organs, because the brain cannot be examined while living, and furnishes no certain evidence of its having been the seat of disease, after death? Are we to say, that it is no assistance or advantage to us in our search for remedies, to view the brain or medulla oblongata as the part in which the morbid symptoms originate; or that we can derive no practical good from remedies which such a view of the relation of those symptoms might suggest?

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* It would be interesting to learn, suppose it was universally admitted that spinal irritation arose from neuralgia, how far removed from "shadowy and indistinct" our knowledge of the pathological state of the spinal cord or its membranes would be. In what condition should we conceive the cord to be when affected with pain? Is our notion of it, if we can have any, at once converted into absolute knowledge by the term neuralgia?

If we consider for a moment the amount of our absolute knowlege of the nature of most diseases, we shall, I fear, find that it is much less than we imagine. What, for instance, is the nature of rheumatism? Whence arises its pain? Is it, as the reviewer asks of the pain in spinal irritation, from neuralgia, congestion, or inflammation? If it be from the latter, why is it not always got under by the remedies applicable to inflammation? and if it be neglected, why does it not run into suppuration? It will of course be answered, it is a specific inflammation—an inflammation *sui generis*. And what does *specific* and *sui generis* mean? Simply, that we know nothing at all about the matter; but that we see by the progress and effects of remedies and results, that rheumatic is very different from common inflammation; and this is what is called understanding the nature of a disease! The fact seems to be, that a knowledge of the proximate cause, or even of the pathological condition of the affected part, however desirable, is not commonly necessary to efficient or successful treatment. We may have a very intimate knowledge of the seat, character, and habits of a disease, and of the influence of remedies, without understanding any thing of its nature or pathology.

But the reviewer asserts, that the texture or part in which this irritation resides is unknown, and here we are at issue. "When we examine its seat," he says, "we find pain vehemently excited by the slightest touch, which leads inevitably to the conclusion that it (the spinal marrow may or may not be consentaneously affected) is external to the vartebral canal. It may be in the ligamentous structure; in the nerves after their exit from the osseous cylinder; or even in the common integument." It seems surprising the reviewer should not perceive, that every word I have here quoted may be perfectly true, and yet leave the belief undisturbed, that the irritation or affection so named resides in the spinal cord. No one denies that the pain may be in the ligamentous structure; or nerves, after their exit; or common integument. It is only asserted, that such pain is connected with and dependent on a morbid condition of the adjoining portion of the cord, as it may be that pain in the knee, in disease of the hip joint, is dependent on irritation, or some disturbed condition of the crural nerve in the groin, whatever tissure or structure about the knee that pain may seem to exist in.

Again it is asserted, the highly sensitive state of the system in

patients, generally females, affected with this disease, is calculated to throw doubts on our examinations. For though a person may shrink or shriek on pressing the spine, she may evince the same indications of suffering, if the hand be transfered to another and remote part of the surface. I have occasionally, though very rarely, seen this happen, but, even if its occurrence were frequent, I cannot see how it makes against the conclusion, that the spinal cord is in a morbid condition; or how it even suggests a doubt on the subject, if we recollect that such a sensitive state of the surface only exists in parts corresponding to the part of the vertebral column which is tender to the touch, as in the abdomen or lower extremities when there is great soreness in the lumbar vertebra or sacrum, or in the chest or upper extremities when in the cervical or dorsal. And why should not irritation or disease at the great trunk of the nerves occasion increased sensitiveness in all their minute extremities, to the finest fibril as well as in any particular one? It seems to be insinuated, however, that the reports of patients in this state of nervous disease are not much to be depended on, especially if leading questions be put to them; but can it be for a moment supposed that those gentlemen who have been investigating this subject, with such care and accuracy, were thus misled: or that they would have ventured to claim the attention of the profession for opinions, founded on the misstatements of hysterical females. For my own part I required no other proof of the sincerity of my patients than the knowledge of anatomy and of physiology which they displayed. When a girl had pain of stomach, or at the sternum, (I mean in cases when these were the only symptoms present, and the whole spinal column was not tender,) she never complained as I ran my finger from above downward until I came to the seventh or eight dorsal vertebra; nor, if her complaint was only at the umbilical or pubic region, until I touched the lumbar vertebra. If I examined the spine of another without knowing what she complained of, she never told me she had pain at the bladder when I pressed the cervical vertebra; nor at the neck when I pressed the lumbar; her report corresponded with the proper distribution of the spinal nerves issuing from the part pressed on. But I will give a table of 148 cases published in my work on Spinial Irritation, and, if not supposed to be all fictitious, they will afford matter for a few questions.

SUMMARY OF CASES OF SPINAL IRRITATIONS.

	CASES.	PROMINENT SYMPTOMS.
A	28 cases of cervical tenderness : 8 men. 8 married women. 12 unmarried.	Headach, nausea or vomiting, face-ach, fits of insensibility, cough, dyspnœa, affections of the upper extremities. In two cases only, pain of stomach. In five, nausea or vomiting.
B.	46 cases of cervical and dorsal tenderness : 7 men. 15 married women. 24 unmarried.	In addition to the foregoing symptoms, pain of stomach and sides, pyrosis, palpitation. In thirty-four cases, pain of stomach. In ten, nausea or vomiting.
C.	23 cases of dorsal tenderness : 4 men. 6 married women. 13 unmarried.	Pain in the stomach or side, cough, oppression, fits of syncope, hiccup, eructations. In one case only, nausea or vomiting. In almost all, pain of stomach.
D.	15 cases of dorsal and lumbar : 1 man. 11 married women. 3 unmarried.	Pain in the abdomen, loins, hips, lower extremities, dysury, ischury, in addition to the symptoms attendant on dorsal tenderness. In one case only, nausea.
E.	13 cases of lumbar tenderness.	Pains in the lower part of the abdomen, dysury, ischury ; pains in the testes or lower extremities, or disposition to paralysis. In one case only, spasms of stomach, and retching.
F.	23 cases, all the spine tender : 4 men. 4 married women. 15 unmarried.	The symptoms of all the foregoing cases combined.
G.	5 cases, no tenderness of spine.	Symptoms resembling the foregoing.

In all making 148 cases ; twenty-six of which were males, forty-nine married women, and seventy-three girls.

Now, why is it, I would ask, if the complaints of patients suffering with spinal irritation are so fanciful, that none of those twenty-eight who had tenderness of the cervical vertebræ, or of the forty-six who had tenderness of the cervical and dorsal, or of the twenty-three who had tenderness of the dorsal only, by chance or design, never complained of pain in the lower part of the abdomen, loins, hips, pubis, or lower extremities, or of ischury, or dysury, or hysteralgia? And on the other hand, why is it that none of the thirteen patients affected with lumbar tenderness complained of nausea, or pain of stomach, or cough, or oppression, or affections of the upper extremities.*

Leaving these queries for consideration, I may yet notice that the reviewer is in error, if he imagines those who advocate the doctrine of spinal irritation were led to its adoption by the discovery of tenderness of the spinal column. This tenderness is found to be very generally an accompanying symptom of the disease, but by no means a necessary one. It is wanting in many cases in which the result proved organic disease of the cord was going on, and it is sometimes absent in cases of mere irritation, not only in those in which the internal affection has continued after the tenderness has been removed by remedies, but in which it never at any time existed. Thus we often find in cases manifestly of this nature, where yet there is no soreness of spine, that pain of side, or chest, or abdomen, or cough, or oppression, is produced by pressure on a particular vertebra. Tenderness of spine is a symptom of value as regards some point of diagnosis, but the doctrine and chief proofs of the existence of a state of disorder in the spinal cord called irritation, preceded its discovery and are independant of its presence.

* Dr. Marshall thinks the distinctions here drawn, in which certain affections are referred to certain portions of the cord, unnecessary, and not always correct; for instance, he has met with cases in which the patient complained of the chest or upper extremities, when the tenderness was only in the lumbar vertebrae. I very much doubt whether Dr. Marshall has come to this conclusion with sufficient accuracy of observation. He may have been led into a mistake by the fact, that when once the whole spinal cord has been engaged in disorder, as in protracted cases, although in the progress of such cases the tenderness may diminish or disappear at one point, and becomes more acute at another, the whole cord is still in an over-excited and unhealthy state. Pressure then on a lumbar vertebra will frequently produce symptoms affecting the head or chest, even where there is no cervical or dorsal tenderness.

It is in the physiological laws of the nervous system, and in physiological reasoning, that the main evidence for the doctrine is to be found. Without entering very deeply into the subject, I may refer to the admitted fact, that irritation or disturbance at the trunk or origin of a nerve, manifests itself not so much at that trunk or origin, as by pain, or disorder of function at its extremity or in the distant organ to which it is disturbed. This is perfectly familiar to us when patients complain of pain in the knee or ankle. If there be no very manifest sign of disease in these parts, we at once recollect the physiological law referred to, and make pressure behind the great trochanter, or in the groin, to ascertain the state of the hip joint, and whether there be any cause of irritation there affecting the trunks of the nerves. And why, let me inquire, in the name of common sense and fair reasoning, when patients complain of pain in chest or abdomen, and we have any ground for doubting the existence of internal organic disease, do we not think of the spinal nerves, and make pressure in like manner as near the trunk as possible of the pair or set distributed to the pained part? Simply, because we have never been in the habit of doing it; and for no other reason that man can conceive!

I might rest the whole case on what I have just stated, but is there really no weight at all in the striking analogy existing between the disorders occasioned by irritation, and those by organic disease of the cord? In what we suppose to be irritation of the cord, we have simulations of every known disease of every organ of the body, and in its organic disease I need only refer the reader to Abercrombie's work to shew that the same results are observable. The close resemblance of the phenomena presented by organic affections of the cord in particular instances to those supposed to be occasioned by irritation, is however, much more convincing, than any offered to us in the general similitude of character in all of them. But to such I can now only briefly allude.

One of the reviewer's principal objections to our doctrine is derived from the inefficacy of local treatment, and certainly he has put this forward in a way, which it would require no common ingenuity to answer, assuming the inefficacy, whether the patient be cured by the local treatment or not. If the patient be cured simply by local bleeding or counter-irritation near the spine, he says it is often so difficult to distinguish between physical amelio-

ration and mental impression, that such evidence, is to say the least of it, of a very doubtful validity; and if cured by this treatment, in conjunction with remedies of a more general nature, as tonics, change of scene, air, &c., the darkness becomes still more penetrable, or the little light that shines is unfavourable to the doctrine, I suppose because, if local treatment was of any avail, the other remedies would have been unnecessary! Is this the manner John Abernethy would reason on the cure of an ulcerated leg, or that Sir Benjamin Brodie would argue on the treatment of a rheumatic knee joint. That I may not be charged with misrepresenting the reviewer's sentiments, I quote his words towards the conclusion of his critique. "Dr. Marshall," he says, "has erred in not limiting his measures to the assumed great original of all the symptoms *and thus proving the doctrine;* or he has erred by not omiting those local measures, *and if successful thus aiding to disprove it.*" Clearly assuming it as an established principle that local diseases are to be cured by local treatment alone, and that if it be necessary to employ general remedies, there must be great doubt whether any disease could have existed in the part supposed to be affected! If this principle was applied to the cure of a severe rheumatic affection of the eye or of the knee joint, we might, without any violence, on witnessing the failure of our leeching and blistering, and the success of a few doses of colchicum, believe, that the sclerotic coat in one case, and the synovial membrane in the other, were in the most perfect health; and in short that we had made an utter mistake in supposing there was any disease of the eye or of the knee whatsoever.

To take even the extreme cases put by the reviewer, in which local remedies do no good whatsoever, but where constitutional treatment is attended with benefit, and I admit very many such are to be met with, what, I would ask, is the legitimate inference? May it not be answered, that although the efficacy of local treatment is some evidence of the seat of a disease, its inefficacy, or the after success of constitutional treatment, proves absolutely nothing on the subject, except the obvious fact stated, that a disease, assumed to be local, has yielded to constitutional treatment, after having resisted local remedies. That irritation of the spinal cord, viewed as a local disease, is not singular in this respect I need offer no other proof, than those given by Mr. Abernethy in his work

"On the Constitutional Origin of Local Diseases." To refer, however, again to rheumatism, are we to infer, when we have cured affections of the hip, knee, or ankle by colchicum, turpentine, or guaicum, after the failure of local remedies, that no disease whatsoever existed in the hip, knee or ankle?

If it was not a melancholy consideration to any humane mind it would be amusing to watch and contrast the endless changes and fickleness of medical opinion from year to year. Mr. Abernethy made his name celebrated through Europe by directing the attention of medical men to the influence of constitutional treatment on local disease, till local remedies fell almost into utter disuse. But now the effect of local remedies is made the test by which we are to determine whether there be any diseases of the part supposed to be affected or not; so certain is the influence of unaided local treatment deemed to be!

I am disposed frequently to refer to rheumatism in illustrating by analogy the habits or character of spinal irritation. There is a strong resemblance in many instances between the two complaints; so much so indeed, that the latter is frequently mistaken for and treated as the former. Spinal irritation, like rheumatism is often very limited in extent, and strictly a local disease; in, such cases it is almost always benefited by local treatment. In other instances it is more general in its attack, affecting nearly the whole of the spinal cord, and, like general rheumatism, partaking more of the character of a constitutional affection. In the early stage of such an attack, much may be done by judicious general and local treatment in either disease, but if, in consequence of its extreme violence, or of mismanagement, it runs on until the system becomes thoroughly imbued with it, or until it assumes a chronic form, it becomes altogether intractable, In this state, however, there is a manifest difference between rheumatism and spinal irritation as regards the facility or difficulty of cure. In the former complaint we are acquainted with several general remedies which are known to possess a specific influence over the prevailing diathesis. In the latter we as yet know of none, and are accordingly compelled in such cases to depend on symptomatic treatment, combined with strict attention to improvement of the general health. In all cases of spinal irritation indeed, as of rheumatism and most other diseases, the cure is best attained by a judicious combination of both local and constitutional remedies;

and if this be "polypharmacy," as the reviewer designates it in contempt, the term and the reflection applies more forcibly to the present state of medical science generally, than to any which has been advanced on the subject of spinal irritation.

In freely admitting the frequent inefficacy of local treatment alone in cases of protracted spinal irritation,* the experienced reader will at once perceive I only assent to a fact, true of almost all chronic diseases, and especially true of functional affections, or to speak more clearly, of chronic disorder of the nerves of organs. How often have I not seen tormenting dyspepsia, or chincough, after resisting all direct remedial measures, banished, as if by a charm, on mere change of air. How wonderful have been the recoveries I have seen it effect even in mesenteric disease, and in affections of the lungs under similar circumstances; and is the influence that so remarkably alters the functions of organs, and so strangely affects the structural changes going on in them, to be set aside in the cure of disorders of the great trunks from which they are supplied with nerves? Or if, on the other hand, it cannot be set aside, must we conclude that the trunk is not the seat of disease, nor any other organ or part of the human frame that we know of?

In addition to anything I have already offered in favour of the doctrine of spinal irritation, it seems to me to be no slight evidence of its truth, that it has explained to us the dependence of certain symptoms not understood before; and furnished us with new and more correct means of forming our diagnosis in many diseases. In some affecting the chest or abdomen, for instance, in which pain or tenderness on pressure is a principal symptom it is of importance to know whether this pain or tenderness be superficial or deep-seated—be in the spinal nerves distributed to these parts, or in the

* It seems to be altogether overlooked by the reviewer, that spinal irritation is very generally, if not always a symptomatic disease. In highly nervous temperaments, and where there is a strong hereditary disposition, it may occur as an idiopathic affection, but it more usually arises from disorders of the digestive or uterine functions, or from chronic structural disease. When arising from the latter cause, which is not probably removable, the spinal irritation is necessarily incurable, however limited in extent it may be. Of this nature are the neuralgic complaints in phthisis, diseases of the heart, abdominal tumours, caries of the vertebrae, &c.

deep-seated viscera; and this knowledge is now as easily attainable by pressure on the spinal nerves, as a knowledge of the cause of pain in the knee may be by pressure behind the trochanter, or in front of the hip-joint. When there is tenderness of abdomen in fever, or in presumed inflammation, now we may at once ascertain whether it be a formidable symptom or not; when there is pain below the breast, or in the side, or at the sternum, we can, without reference to the stethoscope, state, whether it be symptomatic of phthisis, or any other affection of the lungs; and when there are palpitations, oppressions, faintings, angina pectoris, and other symptoms of diseased heart, we can very generally set the patient's mind at ease on the subject; and all this by a simple examination of the spinal column.

For myself I cannot express the advantages which I have derived in forming a diagnosis in obscure cases, from the study of this subject. I have at the same time witnessed, and am daily witnessing my seniors in the profession, men of high qualification, of long experience, and eminent in reputation—men "of that very thinking and practical class," who, it is stated by the reviewer, remain uninfluenced by the doctrine I have discussed, committing the most egregious mistakes with respect to the nature of the diseases they were called upon to treat, and which the slightest knowledge of that doctrine would have revealed to them. One gentleman was treating a lady for some nervous symptoms, with debility, tightness of chest, yawning, &c. She happened to faint during his visit on some occasion, and he was removing her to the sofa, when she gave a loud scream. Her back it appeared had been hurt by the the pressure of his arm in supporting her. This led to an examination of the spine, when some of the vertebræ were found so exceedingly tender, that the doctor thought that there must have been a caries of the bones, and believed he had now discovered the whole cause of his patient's general delicacy. Issues and a recumbent position were immediately ordered; but she went from bad to worse, until her friends became so alarmed, that they took her to Dublin, where the affection was understood, and treated properly. Another medical gentleman, in high repute, had been for years tormenting the mother of a young family with leeches, blisters, tartar-emetic ointment, and digitalis, for disease of the heart. She suffered dreadfully at times with palpitation, oppression, and other nervous symptoms, which were little benefited ei-

ther by the medical treatment, the confinement, or the rest to which she was subjected. This poor lady had the nature of her complaint fully explained to her; could tell what hypertrophy was, and how the little valves at the mouth of the aorta had ossified, and almost shut up the ventricle. She could actually hear the whizzing of the blood as the heart endeavoured to project it through the contracted chink, and entertained little or no hopes of recovery. After years of suffering had elapsed, her husband's affairs, fortunately for her, obliged him to remove to a distant climate. She accompanied him of course, but not without much misgiving at being compelled to leave the doctor behind her. Wonderful, however, to relate, before she had been long on her voyage, the valves began to return to their natural state, the increased size of the heart to diminish, and all the disagreeable symptoms which had annoyed her so long to disappear! A third medical gentleman was called to a case of presumed inflammation of bowels. He bled and gave relief; the complaint recurred, and he bled again largely; again relief was obtained, and again there was a recurrence even to a more violent degree than before. Eventually the case ran into a form of chronic nervous disease, from which she did not recover for months. It was one of pure irritation of the spinal cord.

Very lately a lady came from the country to consult me about a pain in her side and upper part of the chest, sometimes under one clavicle, sometimes under another, and sometimes at the sternum. She suffered so much, that she frequently passed successive nights sitting up in the bed, the pains being made worse by the recumbent position: they were occasionally accompanied by tightness of chest and oppression. When she was first attacked, about six months before, the pains were more general, affecting the extremities, and resembling rheumatism; for which complaint, after some treatment, she was sent to the seaside to take hot baths. After taking some baths, she became much worse, and was seized with violent pain in the chest, cough, and general illness. For these symptoms she was bled and blistered, which gave considerable relief, but in five or six days the complaint returned almost as severely as before. The physician who had bled her now informed her that her lungs were affected, and that if she did not immediately leave the sea-side, she would run into rapid consumption. On returning home, she saw her former medical attendant, who

would not admit that her lungs were affected, but said she was labouring under chronic bronchitis. These differences in opinion induced her to come to Limerick and consult me. Though she had been ill nearly half a year, I found her pulse perfectly natural, her skin cool, her tongue clean, and but little cough: her appetite was pretty good; in fact she said she would be quite well only for the pains which tormented her so continually, that she feared she would never get rid of them. She told me she had been two or three times attacked with a swelling in the neck and upper part of the chest since this complaint affected her; that it was said to be erysipelas by the medical attendant; and lasted four or five days. On examining the spine, I found the lower cervicle vertebræ, and almost all the dorsal, exceedingly tender to the touch; some were more so than others, and when pressed on, the pain shot forward to the part at which she usually complained. The lady was about fifty years of age, and had passed her catamenial period; her bowels were disposed to be confined, unless she took medicine: when not in pain she slept well. I directed an application of leeches to the spine twice, at intervals of three or four days, after which she was to take a quinine mixture two or three times a day, and rub tartarized antimonial ointment to the back. She became better after the leeching and ointment than she had been for months, and in a fortnight felt quite recovered. At the end of two or three months, however, she got a relapse, and came to town again to me. She was now leeched and blistered, after which I directed a plaster of opium, camphor, and belladonna to be applied to the chest and spine; and a mixture of quinine and valerian to be taken three times a day. In a week I sent her home nearly well again.

Even though there should be frequent relapses in cases of this description, it is to be attributed rather to the early error in treatment, than to the inefficacy of that which is eventually resorted to. I have always remarked that if these nervous attacks are not arrested at the very onset, they are apt to become a sort of habit with the constitution, and to return as periodically as sleeping or waking. When they are of any considerable standing, it is indeed only by the utmost attention to the general health, change of place and air, with perhaps local friction or other such remedies, that any success can attend our efforts to remove them.

Before concluding this paper, I may remark, that the reviewer

speaks of the doctrine of spinal irritation as a *theory;* and perhaps, as it applies to particular cases, it may be considered so. It is, however, not advocated solely in reference to any particular one, or any ten cases, but to a multitude of undeniable facts which are adduced in proof of some disorder, call it irritation or what you will, existing in the spinal membranes. I do not, indeed, wish to see the opinions I have offered received any farther than they can be considered a pure doctrine of facts.

It is a fact, that there are disorders incident to the frame which we have not heretofore been able, satisfactorily, to refer to any particular organ, but all the phenomena of which are now readily explained, by supposing the morbid cause to exist in the spinal cord or its nerves.

It is a fact, that we might infer the possibility of such disorders, in disease of the cord, from our knowledge of its physiology, even though they had never occurred. Mr. Abernethy, in fact, inferred their occurrence merely from the discoveries of Le Gallios.

It is a fact, that affections of the cord will produce all the several disorders attributed to spinal irritation, as any person may convince himself, by reference to Dr. Abercrombie's work on diseases of the brain and spinal cord.

It is a fact, that in these disorders in which there exists such strong presumption of spinal disease, tenderness of the spine, on pressure, is almost invariably found; that such tenderness very commonly corresponds with the situation of the trunks of the spinal nerves, distributed to the distant, pained, or affected parts, that pressure in such situation very often excites or increases the pain or affection in those parts; and finally, that remedies applied in such situation often give very immediate relief, and sometimes effect a permanent cure.

PROBLEM V.

BY D. GRIFFIN, M. D.

UNDER WHAT CIRCUMSTANCES, AND TO WHAT EXTENT, IS BLEEDING PROPER IN DISEASES OF THE BRAIN?

Dr. Cheyne, in his work on apoplexy, thus writes—"I am desirous of removing every objection which can be opposed to blood-letting, which I am convinced is not only the most effectual remedy in apoplexy, but is much more effectual than all the others in use." In another part of the same work he speaks in the following terms—"Two pounds of blood ought to be removed as soon as possible after the attack, and if the first bleeding has not been of service, and the disease is unequivocally established, the chief question to be decided is the additional quantity of blood to be drawn. It ought to be known that from six to eight pounds of blood have been taken from a person by no means robust, before the disease, which ended favourably, began to yield. The first and second blood-letting ought to be large, and a third ought to follow the second as soon as it is ascertained that this has been ineffectual in stopping the disease." Again, he goes so far as to say—"I believe it is a good rule *to have every patient in apoplexy who is not plainly dying, blooded,*" and in another passage, relating to the same remedy, he is more absolute still. I quote from memory, but I think his expression is—"Should these means fail *the patient will stand in need of such attentions only as the dying require.*"

Be far the greater number of our modern writers have in general terms strongly recommended the same remedy. Among the most strenuous advocates of it may be mentioned John Hunter, Cullen, Pitcairn, Cooke and Abercrombie. It is not necessary to give

extracts in support of the practice from each of these authors. I shall confine myself to the first and last named, to shew the extent to which the remedy was recommended. John Hunter says —"The only difference between apoplexy and hemiplegia is in degreee for they both arise from extravasations of blood." In these cases he thinks—"We ought to bleed very largely, especially from the temporal artery, till the patient begins to shew signs of recovery, and to continue it till he might begin to become faintish."

Dr. Abercrombie says—"Our first great object is to take off the impulse of blood from the arteries of the head, by bleeding carried to such an extent as shall powerfully and decidedly affect the system, and by repeating it at short intervals as soon as these effects begin to subside. The first bleeding should probably be from the arm, but after this there seems to be an evident advantage in abstracting blood locally, either from the temporal artery or by cupping." He says afterwards—"In the extent of our evacuations, indeed, a due regard is certainly to be had to the age and constitution of the patient and the strength of the pulse; but I think we have sufficient ground for saying, that there are no symptoms which characterise a distinct class of apoplectic affections, requiring any important distinction in the treatment, or in other words, a class, *which in their nature do not admit of blood-letting*."

In contradistinction to these recommendations we have the following observations on this mode of practice from other authors. To begin with the earlier modern writers, Dr. Fothergill says—"Bleeding in apoplexies is one of those operations which on several accounts requires the most dispassionate consideration. In no disease, perhaps, is the judgment of the prescriber of more consequence to the patient. If it is successful—if the patient recovers upon it—it is a fortunate event for both. If bleeding is performed when it ought not, either death ensues or an incurable hemiphlegia." In another part, after assuming some circumstances in which it may happen to be useful, he says—"It is possible, likewise, that by a copious bleeding the animal strength may be so much reduced and the effort began so powerfully checked by the operation and the effects of the disease itself, that the patient expires soon afterwards or survives a few days and suffers a hemiplegia, *none of which might probably*

have happened had bleeding been omitted. It becomes the operator, therefore, most carefully to attend to every circumstance of his patient's situation before he opens a vein *which may, perhaps, be decisive of his patient's fate.* Again he says—"It seldom happens that a physician arrives before this operation is performed, let the disease have originated from what cause it may, and though very few would probably recover if this operation had been omitted, yet comparing what has happened to those who have been blooded and the few I have seen who have not, I am of opinion, that bleeding in apoplexy is for the most part injurious, and that we should probably render the most effectual aid by endeavouring in all cases to procure a plentiful discharge from the stomach and bowels, as by these revulsions, the head is perhaps much more effectually relieved from plenitude, and that without weakening or interrupting any other effort of nature to relieve herself, than by venesection." Dr. Heberden, though he does not interdict bleeding, yet disapproves of "large, repeated, indiscriminate abstractions of blood, which experience proves to have been often prejudicial." Dr. Kirkland says—"It may be observed, that a loss of blood, to whatever extent carried, affords not any relief in the vehement apoplexy, and yet much dependance has always been had upon bleeding,—indiscriminately, in every disease which has been called apoplexy." Dr. John Brown considers apoplexy as a disease of indirect debility, and therefore does not admit the evacuation of blood into his list of remedies, and Dr. Darwin says, "copious venesection must be injurious by weakening the patient."

Not to multiply instances I proceed at once to one of our most modern authors, in whose writings there is a very rational consideration of all the facts relating to apoplexy. I mean Dr. Copland, who says, in treating the subject in his Dictionary of Practical Medicine—"The treatment of apoplexy has long furnished subjects for discussion, not only as respects the more subordinate means of cure, but also as regards the most energetic measures, and the intentions with which they should be employed. This is evidently owing to the difference which has long been acknowledged to exist in the pathological states constituting the disease, but which has recently been questioned. Without recurring to the changes so fully described above, I may remark, that a person is seized with apoplexy, and instead of being blooded is treated

with stimulants and restoratives, and yet he recovers without paralysis having supervened. Another person is blooded largely and he recovers. A third is treated in a similar manner, and he becomes hemiplegic in the course of the attack, and a fourth is also blooded and he dies.—Now these are very common occurrences, and point to very important considerations which I will pursue a little further. A thin, spare and debilitated man staggers as he walks and falls down in the street, with pale countenance, feeble pulse, and laborious or slightly stertorous breathing. He is blooded by the nearest medical man almost immediately and recovers. A large man of a full habit and lax fibre, suddenly becomes apoplectic, and is instantly treated with stimulants, and volatile substances held to the nostrils, and his consciousness and voluntary motion are restored in a few minutes. One practitioner of experience states, that he never draws blood from a patient in apoplexy, excepting under peculiar circumstances, and avers that he is more successful in his treatment than those that do. Another considers that when one full blood-letting fails of giving relief, no benefit will be derived from pushing it further, but much risque of giving paralysis. A third physician, equally eminent and experienced, confides in blood-letting almost solely, and carries it often to a great amount, aud a fourth, whilst he discards depletion, trusts to stimulants chiefly."

Such instances are sufficient to shew the variety of opinion that prevails on this important matter. The public is apt to consider bleeding by far the most powerful and appropriate remedy in this disease, and if a young practitioner was led by facts within his experience to doubt its efficacy, and had the firmness to act on this doubt, the feeling in favour of it is so universal that he would be likely to fall into some discredit if the case ended fatally. After the extracts I have given, I need not indicate how little support he would obtain for any opinion his experience had led him to, either for or against the practice if he chose to betake himself to study to escape his difficulties.

The pathological discoveries of the last few years have satisfactorily accounted for the marked difference that appears between writers of authority on this subject. All these differences would have been easily accounted for if it had been known that effusion —extravasation of blood—softening or hardening of the cerebral substance—abcess—deposition of false membrane, or almost any

form of organic disease may exist in the absence of those symptoms which are usually supposed to characterise it, and that, on the other hand, every symptom may be present that is generally looked upon as indicating the presence of one of these conditions, and yet that condition be found wanting after death. Hence, while people were guided by symptoms alone, without considering or indeed without knowing the extent of disease they might sometimes involve, and while bleeding was a remedy almost universally used on the sudden suspension of the functions of the brain, it was no wonder this remedy should be found beneficial or injurious according as a less or greater extent of organic disease existed, and thus a physician's opinion with regard to this remedy would be favourable or otherwise, according as he happened to be thrown more frequently upon cases of the one class or the other. It is a result of later and stricter modes of enquiry in medicine, that symptoms cannot be depended upon as indicating the particular place or the actual amount of any cerebral disease. It is possible that when these methods have been persevered in for some time our powers of distinction in these matters may be strengthened; but though the mass of cases of this class on record is so abundant as to be absolutely overwhelming, the facts of each case have not been noted with sufficient accuracy, and more particularly, they have but seldom on the whole been followed by examinations after death, on which alone the certainty of our conclusions rests. It must, therefore, be confessed, that however desirable it may be to pronounce upon the existence of any particular pathological condition during life, there is, really, no means of doing so. The distinctive marks of particular states do not even amount to strong probability, much less to that well grounded conviction that would justify the use of powerful remedies, applicable, perhaps, to no more than one variety of organic lesions and very injurious in all the rest. The more modern writers, who fully admit all this, still continue to enumerate certain groups of symptoms as strongly characteristic of particular states. I need not say how useless this is, while they admit, as they almost always do the uncertainty of the conclusion towards which they are driving. It is even mischievous, because such groupings of symptoms, when unaccompanied by statistical returns, have always a greater weight than they are entitled to in the mind of the medical student, which is directed more to the point to which they tend than to

the caution by which they are accompanied. Besides, there is a source of uncertainty which is not hinted at at all. As far as I have been able to observe, the symptoms said to be indicative of a particular state are collected rather from a consideration of the functions such a state might be supposed to interfere with, than from facts drawn from cases in which that condition only was found present after death. This arises from the circumstance alluded to above—the small number of cases in which all the facts before and after death have been observed with sufficient accuracy to found conclusions on them, and is only blameable inasmuch as it occurs with persons who admit the value of the numerical method and the uncertainty of mere opinion, and yet, in this instance, seem to forget their conclusions and fall into the old error again.

In directing attention to the utter impossibility of inferring, from any group of symptoms, the precise seat or the degree of any cerebral disease, I am sensible that I only point to a difficulty without shewing the means of escaping from it ; yet, there is one conclusion of immense practical importance that I think will be found correct, namely, that in considering the expediency of bleeding *the actual degree of organic disease that exists* is of infinitely more importance than its seat or nature—in other words, that bleeding is badly borne in all cases of extensive organic disease of brain, *wherever the disease may be situated or whatever be its nature.* I do not deny that situation may be of much consequence in questioning the value of this remedy —our knowledge of the functions of some parts of the brain would tend to shew that it is so—but we are not yet in possession of facts sufficient to prove its relative importance, and even if we should be able yet to ascertain this, the tendency of such a discovery would only be to narrow the limits of those cases in which bleeding is useful. The great mistake made about bleeding in diseases of brain is looking to the fit as the test of its utility, instead of looking to the whole course of the disease and the event, and comparing cases in which it is used with those in which it is omitted, in all their circumstances, more particularly the last. Nothing can be more obvious than that the duration of the fit is a most fallacious test. There can be no doubt that patients would recover from the fit itself in all ordinary cases, whether they were bled or not, and even in many severe ones its

utility, if not its necessity, admits of great question. What is it, I would ask, that produces the fit, and then the speedy recovery in many cases that occur in the street when a man falls down apoplectic, and in a few minutes regains his senses and walks home with or without assistance? If it be said that it is a congestion in some part of the head, it is plain that nothing could be more unfavourable to the occurrence of such congestion, than the position the man was walking in, and that when he has fallen down nothing can be more unfavourable to its removal than his new position. How often does it really happen, notwithstanding all the haste with which the medical man is summoned to the bedside in those cases, that he overtakes his patient in the very depth of the fit? Nay—how very seldom does it happen that the patient has not recovered some degree of consciousness even before the physician can resort to bleeding, though he has been laid in bed in a position more favourable to congestion than almost any other. To take a parallel case. We know that the action of the heart and arteries depends for its continuance upon the performance of the respiratory function—yet, what should we think of a person who would assert that every excitement or suspension of the circulation depended upon changes in the respiratory function. In this case we are able to disprove the assertion by shewing that the respiration is often quite unchanged amid these changes. In the other we have, it is true, no means of proving that there is no extraordinary congestion in any part of the brain, but the assumption that every alteration or suspension of its functions depends upon an altered condition of the cerebral circulation, will, I think, be considered fully as extravagant upon a fair consideration of all the facts connected with the fit and recovery in such cases. It ought never be forgotten that though a certain condition of the circulation in the brain is essential to the manifestation of its functions, yet, when once its action is set up, there is a certain independence about it, and its functions are exercised and capable of undergoing exaltations and depressions subject to other stimuli, though the cerebral circulation may maintain an even current, and it would be as absurd to say, that every expression of the feelings in the countenance—every look of joy, or sorrow, or contempt, or anger—itself a manifestation of the exercise of the proper function of the brain, was dependant upon an altered state of the cerebral circu-

lation, as that every other exaltation or depression, or even suspension of its functions, was *always* so. I may appear to have entered into these observations at unnecessary length, but I do so because I think the assumption that changes in the state of the circulation or " determinations of blood to the head," as they are called, will account for most of the interruptions of function in the brain is a very dangerous error, and I am convinced that many persons have been over bled in consequence of it. The cause of the error is manifest. There are many cases of apoplexy in which large bleedings are not only useful but highly efficacious. Most of these, I suspect, are cases arising from affections of very recent origin, in which but little damage or disorganization has been effected in the brain.—The organ has undergone little if any structural alteration, and bleeding will be borne as well in them as it could be in the most healthy person. The advantage derived from it in these cases is so manifest, that the remedy is extended to all. If mischievous effects follow they are attributed to the inevitable progress of the disease, and thus the error is perpetuated. A case that bears a remarkable resemblance to these is the one of poisoning by opium. Here we have a perfect apoplexy, as far as symptoms are concerned—stertorous breathing—insensibility—immoveable pupils—a laboured circulation and imperfect oxygeration of the blood—but the brain is uninjured by disease, and accordingly bleeding will be borne in such cases to almost any extent and with remarkable benefit. This seems to strengthen the conclusion I am endeavouring to enforce—that the amount of disorganization is the principal source of danger in the employment of this remedy. I need not advert again to the numberless cautions that are given against the indiscriminate use of it. Some of them I have quoted in the beginning of this paper, and I observe that even the most strenuous advocates of whatis sometimes called a heroic* practice, make admissions occasionally that would lead one to suspect them of having

* I cannot help noticing the absurdity of the term " heroic," as applied to these things. We hear of " heroic bleedings," " heroic doses of calomel," &c. Our notion of heroism always includes some danger—often a great degree of it ; but we are not in the habit of giving any man credit for heroism in a transaction of which all the danger is bore by another party. The physician, however much identified with his patient in feeling, is certainly not so in person or constitution,

fallen into the errors of the French school, by founding their curative measures more upon appearances observed after death than upon the circumstances that followed the administration of remedies during life. Thus, I find Dr. Abercrombie—one to whom the profession is indebted for the first specimen of any thing approaching to strict investigation in these diseases—saying of purging, "This is always to be considered as a most important and leading point in the treatment of apoplexy, and though, in arresting the progress of the disease, our first reliance is upon large and repeated bleeding, *the first decided improvement of the patient is generally under the influence of powerful purging.*" Surely this is an admission of immense importance. The cautions given by medical writers on this subject, though numerous enough, will be always ineffective so long as they are unaccompanied by any principle by which to shape our course. The rule that would suggest itself is—to examine in all cases what evidence there is of the existence of much organic disease, and if there is reason to suspect that it is present in any considerable degree, to be very cautious in the abstraction of blood. Indeed, where loss of power is already apprehended, the system should never be so much lowered as to interfere with the process of reparation. It ought also be a rule to trust rather to moderate bleedings repeated at proper intervals, than to run any risk of inducing debility in the after part of the case by taking a large quantity at once. Indeed it appears to me that the effort ought always to be to reduce the circulation to its natural condition, and that it is by no means a direct way of attaining this end to run into the opposite extreme, and resort to large bleedings by way of making, as it is said, a powerful impression. The danger attending such practice and the necessity of the cautions I have given will be evident from the following case which was given me by a friend, and is one of the most remarkable I have met with.

Mr.—, a strong, stout plethoric man, suffered for two or three years from frequent returns of secondary syphilis. The skin had

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and he cannot therefore be entitled to all the glory arising out of the administration of scruple does of calomel or bleedings of forty ounces at a time. The expression is, I believe, of French origin, and seems to be caught up in these countries as a happy one. I observe it is made use of by medical men only. Quere would their patients think it equally happy?

been repeatedly covered with eruptions—spots had ulcerated in several parts of the body, forming deep sores—the throat had been ulcerated—several of the joints successively inflamed. The head and shins had been attacked with painful nodes—the nose had been also attacked, but the affection was for the time arrested. Within the last eight months, after a long interval of tolerably good health, the eruptions on the skin broke out again, and he complained occasionally of pain in the head, which was supposed to be a return of the old syphilitic pains. It was followed after some time by giddiness, deafness particularly of one side and noises in the ears. The giddiness and deafness were very much relieved by the treatment made use of to remove the eruption, and still more afterwards by taking spirits of turpentine and purgatives. As he had a strong objection to cupping and blistering, which was proposed to him, and the turpentine in doses of thirty drops three times a day, with an occasional full purgative and low diet, seemed to give him progressive relief, they were continued until he declared himself quite well, which was about six months from the first complaint of giddiness. Soon after this, he was observed one day to get confused and wander a little in conversation, and fell back off his chair in convulsions. He worked violently for some minutes—then became easier, but breathed loud and stertorously and frothed much at the mouth. The convulsions returned every five or ten minutes for ten or twelve turns, although over thirty ounces of blood had been taken from his arm at one bleeding. At the end of two hours the convulsions ceased, and sensibility was so far recovered that he was capable of some voluntary movements and could articulate. He now began to roar or scream, strike his arms about, and twist his body violently, so that five or six men were necessary to hold him. As this violence was continuing in a frightful way for some time, and his pulse was good, ten or twelve ounces more of blood were taken from the arm, soon after which he became quieter, and his pulse was soft at 90. He was evidently sensible to pain and swallowed a spoonful of water. He now got ten grains of calomel and soon after a purgative enema, which seemed to empty the rectum, but no more. In the course of another hour there was a very considerable amendment. He recovered his consciousness perfectly, swallowed well and became quiet. He had no recollection or knowledge of anything that had happened—won-

dered at the fuss and work that was about him, and asked why his head had been shaved and cold lotions applied to it. His pulse was over 100 and rather weak than otherwise, his bowels had not been moved except a little by the enema. Another, therefore, was ordered, and he got a scammony powder by the mouth. Another was ordered to be given in three or four hours, and the enema to be repeated unless the bowels were freely moved. At night when I saw him again he was quite easy and his senses perfectly restored—but I observed a very disagreeable quickness of respiration about him, and his pulse was yet weaker than before at 120. A blister was applied to the back of the neck, and directions were given if he got any weakness during the night to give him wine and water. Soon after leaving him (as the apothecary informed me in the morning) the pulse became still fainter, and he grew very restless. He got no sleep, but tossed and turned in the bed the whole night—perspiration broke out on his forehead and face—his extremities cooled, and his pulse was scarce perceptible at the wrist. When we saw him early in the morning we found his bowels had been moved freely through the night—he was perfectly sensible but pulseless and anxious, and his respiration extremely rapid. He could not be kept quiet or in one position for three minutes together, but was continually flinging himself from one side of the bed to the other. He was, in fact, precisely in the condition of a person dying of hemorrage, and presented a striking resemblance to cases which I had seen of women with severe flooding after parturition. He got five grain doses of opium, brandy and broth, without the pulse returning to the wrist or the least excitement of the circulation, and expired in about two hours!

The close of this case was very remarkable. The apopletic symptoms passing off so completely and no others being present towards the end, except signs of pure debility, shew that whatever the nature of the cerebral affection may have been, (for there was no examination after death,) the bleeding, if useful at all, was certainly carried farther than necessity required. Yet, it was a case in which, according to the usual mode of thinking, no one would hesitate—a large, strong, and temperate man, with a substantial and full pulse, suffering under an affection of the brain, sudden in its occurrence and violent in its symptoms. These circumstances are of immense importance, as the same

signs of debility and the same result following a bleeding in a spare frame, only moderately strong, would not have furnished so forcible an inference against the excesses of this practice, as such an inference would then be partly accordant with our *a priori* reasoning.

In giving the estimated amount of organic change as a guide in determining the degree to which bleeding is to be carried, I, of course, include all affections of brain dependant upon organic changes, whether apopletic or not. There are, however, other circumstances in which cautious against the use or abuse of this remedy are exceedingly necessary, even though the amount of structural alteration may not be considerable. It appears to me that all affections of the brain or its membranes attended with protracted suffering are cases which bear bleeding very badly. I believe every physician of any experience must have met with instances to convince him of the truth of this—and it is a result one would readily anticipate. Some physiologists assert, that the vital functions are mainly dependant upon the sensorial for their exercise. Whether this assertion be true or not, it is quite certain, that a violent and sudden impression on the sensorium is capable of producing such a sinking of the vital powers that, in some instances, the functions of life will cease at once. Many facts of this kind are on record—a violent and excruciating pain has been known to produce this effect; and it seems to matter little whether the impression produced be one of pain or of pleasure provided it is sufficiently strong. The story of the Grecian who expired suddenly on being told that his children had won at the Olympic games is a case in point. The effect of such impressions on the nervous system might be pursued much further, but it is a sufficient to remark, that the same exhaustion that a very violent and unexpected impression will produce at once, will be produced by a less violent one often repeated or long continued, such as the pain attendant upon any acute disease that occupies some days in its course. I may mention as particular instances those cases of suppuration within the head, in which the symptoms of inflammation often run very high towards the close. In these circumstances, and indeed in all cases of whatever kind in which excruciating pain is a prominent symptom, (for the remark I think applies generally,) whether there is little organic change or none at all, or however violent or high the

symptoms may be—if this state of things has lasted many days free bleeding will not be borne. In fact, long continued agony, independent of anything else, produces of itself a degree of direct debility, which, however it may be masked by inflammatory symptoms, truly exists, and is often as great as if the patient had already lost several pounds of blood. The manner in which death takes place in such cases would lead one to the same conclusion. If we compare their termination with that which marks some of those examples of cerebral disease that occur in full habits advanced in life, that commence with drowsiness and lethargy, almost without any suffering, and end in the gradual supervention of coma, we shall find the difference very remarkable. In the former death often occurs suddenly and unexpectedly, and in many instances long before the symptoms of pressure from accumulated matter have shewn themselves. In others these symptoms have appeared but partially, and the patient retains many of his mental faculties to the hour of death. In others still the patient is perhaps talking to his friends one moment and in the next lays his head on the pillow and dies. In the latter class this is never the case—whether the progress of the case depends upon the gradual accumulation of fluid, or something equivalent to it, there is little or no pain and therefore no exhaustion—death takes place very slowly—function is interrupted after function—the eyelids, cheeks, muscles of the tongue, and soft palate, are paralyzed in regular order—the powers of sense and voluntary motion are entirely obliterated, and last and latest of all the respiratory movements—the former are instances of death by exhaustion—the latter by interrupted function. I shall give a case of the former class, furnished to me by a medical gentleman of much ability, in which the symptoms seemed intense, the bleeding moderate, and yet the result was fatal—

"A man, aged 40 years, was seized with the most intense pain in the head which continued for several days to a degree that nearly drove him distracted. No medical advice was procured for him until the fifth day, when he was found by the physician then called in, with a bewildered unconscious look, but still groaning with pain—he had grinding of the teeth, and the pupils of the eyes were contracted—his pulse was quicker than natural but not weak, and his stomach retained everything which he

drank. The case appeared to the person attending a very hopeless one, but as the only alternative offering a chance of benefit, he took twelve ounces of blood from the temporal artery—shaved his head, and poured a kettle of cold water over the crown and occiput. After directing a blister to be applied to the nape of the neck and the administration of some purgative, he left the house; but had not gone a quarter of a mile from the door when the man expired."

Let me conclude by recapitulating the circumstances in which I consider large bleedings improper with the principles on which they are so:

First.—They are improper, because symptoms are no certain test of the amount of disease, and we may produce a degree of debility (as in the case given) that cannot be contended with.

Secondly.—They are improper in all cases in which extensive disease of brain is known or suspected to exist, as in such cases besides the immediate danger, they produce a degree of debility that would interfere with the process of reparation so far as that is possible.

Thirdly.—They are improper in all cases of disease of brain attended with severe and protracted pain, as such cases usually die, not from any mechanical effect of an existing inflammation, but from the exhaustion produced by the pain that accompanies it.

As a general rule I would, therefore, even in circumstances in which the loss of a large quantity of blood was supposed to be necessary, prefer taking it away in moderate quantities at certain intervals, watching the progress of the case, and being guided entirely by it.

PROBLEM VI.

BY WILLIAM GRIFFIN, M. D.

ON WHAT MORBID STATE DOES THE OCCURRENCE OF COMA AND SUDDEN DEATH IN JAUNDICE DEPEND?

A Poor woman requested me to visit her daughter, Mary Barry, aged twenty years, who, she informed me had been three days ill, and was now speechless, and she believed dying. On entering the cabin in which she lived, I saw her make a faint expiration, which proved to be her last, as she was quite dead when I reached the bed. Her skin was still warm, and universally tinged with a deep yellow colour. The countenance was hydropic, and the pupils dilated. On inquiring, I found the girl's ailment had set in with langour and heaviness; on the second evening she was seized with sickness of stomach, vomiting, and appearances of jaundice, and next morning complained much of her head. She then looked so very ill, that her mother began to get alarmed, and insisted on her going to the dispensary for advice; the poor girl shook her head despondingly, and said she was too weak to walk there, but that she would go into the room and lie down on the bed. These were the last words she uttered; when the mother went in afterwards, there was an appearance of stupor about her, from which she endeavoured to rouse her, but could get no reply? She was in profound coma!

In about three weeks after, I was called to see Ellen Barry, a sister of the former, and found her labouring under an affection precisely similar. She had been attacked with langour and heaviness, followed by sickness of stomach and vomiting, with universal yellowness of the skin. She was now in imperfect coma; conscious when roused, but unable to speak, and very

unwilling to be disturbed. From this very dangerous state she was rescued by active and continued purging; the yellow tinge gradually disappeared, and in a few days she regained her usual health.

Within a very short period afterwards, another member of the same family was attacked; a boy, of about 13 years of age. My brother was requested to see him, and found him moaning and comatose; his belly tender to the touch, his pulse slow, and his skin of a saffron colour; his breathing was not stertorous.—This case was more sudden than either of the foregoing; the boy was seized with sickness of stomach and vomiting at night, and in the morning was jaundiced and insensible. In this state he lay, until nearly the end of the second day, without medical aid, up to which period his bowels had not been moved. An ineffectual effort was then made to purge him, but he was unable to swallow, and died in a few hours.

The parents were now, it may be supposed, highly apprehensive for their remaining children, and the event proved not without just reason. After the lapse of a month their next boy, John Barry, aged eleven years, shewed symptoms of jundice. He grew languid and heavy, and in two days time the tunica albuginea and skin were of a deep yellow. There was a great sluggishness of the bowels, and slight tenderness of the abdomen, but very little pain. He did not complain of his head, but like the others was seized with sickness of stomach and vomiting. I had early notice of this attack, and was vigilant in looking for the supervention of coma, although from any existing symptoms there was no greater reason to apprehend it than in any common case of jaundice, if I except some slight dilatation of the pupils and sluggishness in their movements. The boy was up and about, and did not in fact appear to be very ill; but the fate of his brother and sister left a lesson not to be forgotten, and I accordingly warned the mother to give me instant notice on the occurrence of the slightest stupor,—he was in the mean time actively purged. There was little change in him that night or the next, bnt on the succeeding morning I had a messenger with me at an early hour, to say that he had fallen into a state of insensibility in the night, and could not now be roused. I found him quite comatose, with slow pulse, dilated pupils, and almost a total loss of sensation and voluntary motion. On pinching his hand severely, however, he evinced

signs of consciousness, moaning slightly, and slowly drawing his hand away. Ten ounces of blood were immediately taken from the temporal artery: the head was shaved, and kept wetted with refrigerent washes, and castor oil was administered every fourth hour. As the bowels were slow in acting, injections were given at night, and large blisters applied to the nape of the neck. These had the desired effect. He was copiously purged for several hours, and in the morning evinced signs of returning consciousness; from thenceforward there was, day after day, a steady and progressive improvement, until his recovery became fully established. Some time after his friends were once more alarmed by a recurrence of the vomiting and jaundice: but the progress to coma was arrested, and the complaint readily removed, by full purging alone.

These four cases of jaundice running rapidly into coma, which in two of them terminated in death, when we consider that they occurred in one family,* within a few weeks of one another, and without any unusual or remarkable symptoms which could indicate the impending danger, suggest a very important question with regard to the pathology of the disease: "On what morbid state did the occurrence of coma in these particular instances depend?"

In referring to the works of different authors who have written on the subject of jaundice, it surprised me much to observe, that the occasional supervention of coma and sudden death is scarcely adverted to. This is not noticed even by Cullen or Parr, as a possible termination of the complaint, nor is there any mention made of it in some of the more modern medical treatises,† a cir-

* I have been inclined to think jaundice is sometimes occasioned by certain states of the atmosphere, from its now and then attacking many individuals in the same locality. I was myself suddenly affected with it some years since in common with my servant and many others in the neighbourhood in which I then resided.—These were all under empirical or regular treatment, and recovered in four or five weeks. I took no medicine, except an occasional mild aperient, lived on roasted apples, almost the only food I could use, and was well in three weeks. The cause of the disease is frequently so obscure, that we really do not know what value to attach to medicine, even where recovery takes place.

† Dr. Mason Good speaks of the occasional occurrence of apoplexy in green jaundice only. Dr. Mackintosh, in his New Practice of Physic, makes no mention of it; and the writer on jaundice in the New Cyclopaedia of Medicine is equally silent.

cumstance perhaps scarcely deserving of remark if the occurrence was really rare. We might offer many evidences to prove that it takes place too often to be left at any time out of view in our consideration either of the prognosis or treatment of the disease. Mr. Gilbert Burnet and Dr. Macleod, in a discussion on the subject three years since at the Westminster Medical Society, detailed several cases, which, with few appearances indicative of danger, ran rapidly to coma and death,* and those published by Dr. Marsh in the Dublin Hospital Reports, illustrating the occasional connexion of jaundice with disease of the brain, were probably of a similar nature.† Dr. Gregory, of London, calls our attention to the probability of such termination in severe cases, and Dr. Mason Good speaks of the supervention of apoplexy in green jaundice, chiefly in instances where the pulse was unusually slow. But it is neither in severe instances of the disease, nor in that intense form of it which has been called green, that it most frequently occurs; and nothing so clearly proves how little we really know of the pathology of these affections, than the fact, that the probability of the supervention of coma bears no relation to the intensity of the symptoms. In Dr. Macleod's cases there was little to indicate danger, until fatal coma occurred in one, and epilepsy, followed by coma and convulsions, in the other. In three of the instances which have preceded these remarks there was nothing that could lead one to anticipate immediate danger of any kind, until actual stupor commenced: they were not cases of green jaundice, and previous to the occurrence of fatal symptoms could not even be called severe or unusual.

The general connexion existing between jaundice and certain affections of the brain or nervous system, which attracted the attention of physicians at a very early period of time, and to which the occurrence of the disease from intense passions of the mind has been attributed, while it tends to diminish our surprise at the occasional occurrence of coma, furnishes no clue to the subsisting relation; several explanations have from time to time been offered by those who have paid any attention to the facts, but none that are at all satisfactory. Unfortunately no post mortem

* See Medical Gazette, vol. v. p. 631.

† Dublin Hospital Reports, vol. iii.

examination was permitted in any of the fatal cases which I have mentioned. In one of Dr. Macleod's, that of a young woman who had jaundice for some time, without suffering much inconvenience from it, but who died in forty-eight hours after the supervention of coma, the only morbid appearance observed in the brain appeared to be a deep yellow colour of all the membranes.

Although the yellow colour of the skin and eyes has in these cases been always the first circumstance to attract attention, the mutual sympathy, which is known to exist between the brain and the liver, has led to a very natural doubt as to which might be the primary seat of disorder, and in fact, in this lies much of the difficulty experienced in endeavouring to explain its pathology. Those who have considered the affection of the head as secondary or symptomatic, have attributed it to a plethora of the circulation of the brain, occasioned, like the jaundice, by a gorged state of liver, equally obstructing the passage of blood and bile, or to a supercarbonization of the blood, for want of due elimination by the liver, or to a highly azotised state of it from the same cause, the brain being affected in either case as it is in apoplexy from the circulation of venous blood, or lastly to some such sedative effect of absorbed bile on the cerebral organ, as may be induced by opium or other narcotics. On the other hand, those who have considered the jaundice in these instances, as secondary or symptomatic only, have supposed some oppressed or actually diseased state of the brain, making for some time an insidious progress, and at length manifesting itself by suspending the functions of the liver. An accumulation of bile in the blood vessels, it is said, takes place as a necessary consequence, and precedes, though it be no way necessary to, the termination in coma, which is simply the conclusion of the original affection.

Glancing at these conjectures in the order in which I have stated them, it may be remarked, that no such gorged or infarcted state of the liver has been made out in the cases alluded to, while, I believe, in others where such condition of that organ did exist to an extreme degree, there was no supervention of coma; and again, one should suppose, where the danger depended on pure plethora, the complaint would be easy of remedy, which is by no means the fact. It might, perhaps, be said, this

idea of a plethoric state of the circulation in the brain, derived some countenance from the two recoveries quoted, one by pure purging, the other by purging, bleeding, and blistering : but these remedies are equally applicable to other morbid conditions of that organ.

If the elimination of carbon and azote from the blood be one of the chief offices performed by the liver, its total suspension must necessarily lead to a loaded and deteriorated state of that fluid. That such a state occurs to a certain extent, in almost all cases of jaundice, we have manifest proof in the general languor complained of, the slowness and feebleness of pulse, and altered nature of the secretions. That it sometimes leads to actual coma and death, has been already shown, and that the presence of bile in the circulating system, when artificially introduced, produces analogous results appears from Mr. Phillip's experiments, in which two drachms of bile, injected into the femoral vein of a dog, in a few hours occasioned jaundice, dryness of mouth, vomiting, coma, and death, and in a lesser quantity, effects of the same nature, though less marked.

It would seem superfluous to seek for other proofs of the injurious effects of retained bile, if those I have instanced were any way constant in their occurrence. But that they cannot be received as such, and that they are rather the exception than the rule, must be obvious, when we recollect, how many persons, in whom every texture of the body is deeply imbued with yellow bile, while not a particle passes into the intestines, live for months and years without suffering much inconvenience,* and how infants have grown rapidly, and thriven tolerably, where the hepatic ducts were altogether impervious.† It is clearly unphilosophical to attribute effects in one case to a cause which in nineteen others seems incapable of producing them ; but setting these considera-

* Dr. Gregory mentions that he has seen young persons continue busily engaged in an active employment, their appetite, sleep, pulse, and tongue, remaining healthy, where yet the jaundiced colour of the skin was intensely deep. It was the experience of this fact, probably, that induced Dr. Fordyce to imagine, that the bile was of no use whatsoever in digestion.

† Sir Everard Home has given an example of a child that fed heartily, seemed to digest its food well, and had regular stools, which was, nevertheless, without a gall bladder, or even a duct of any kind leading from the liver to the duodenum.

tions aside, and viewing the question physiologically, there is after all, no just reason for inferring a supercarbonized or azotized state of the blood, from the non-elimination of bile. Tiedemann and Gmelin have made it appear exceedingly probable, "that the pulmonary and biliary organs are, in different tribes of animals, nay, even in different individuals of the same species, in a state of antagonism to one another; that the size of the liver and quantity of bile are not proportionate to the quantity of food and frequency of eating, but inversely proportional to the size and perfection of the lungs." That in fact, as a secreting organ, the liver is chiefly excrementitious, assisting the lungs and cutaneous surface in decarbonizing the blood, and consequently, when interrupted in its function, the duty is merely transferred to the latter, which immediately take on an increased action. To that beautiful relation and correspondence subsisting between all the organs of the body, and especially those engaged in nearly similar functions, we are indebted for the impunity with which we can occasionally suffer the temporary suspension of any of them, and in no instance is this more strongly illustrated than in jaundice, in which the interruption of the hepatic excretions, those of carbon and azote, is met by increased decarbonization in the lungs and skin, and increased excretion of highly azotised principles by the kidneys. Although it seems improbable, that a poisonous state of the circulating fluid can occur in this manner, the occasional consequences of jaundice necessarily countenance the less definite conjecture, that bile retained in the circulation acts at all events in some way or other, as a sedative on the brain and nervous system. To this idea may be traced the frequent application in practice of many influential remedies, and the still more frequent attempt at explaining the most striking phenomena in very obscure affections, by referring them to obstruction in the liver. It is clear, however, we can as yet go no further than to admit some connexion between these effects and their supposed cause, as they bear no regular propor-

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Dr. Blundell records the case of two infants, four or five months old, in whom the hepatic ducts terminated blindly, so that no bile entered the intestines: the stools were white, like spermaceti, and the skin jaundiced; but the infants had grown rapidly and thriven tolerably notwithstanding!

tion to it in their intensity, and are anything but necessary results. We know no more why sedative effects should result from obstruction to the flow of bile in some instances, than we do of their total absence in others.

An endeavour has been made to draw some distinction between cases of jaundice in which the bile is not eliminated by the liver, and those in which it has been secreted and re-absorbed. That such distinction exists and that the former are of a more dangerous nature than the latter, inasmuch as they necessarily include either paralysis, or great disorganization of the organ, no one can deny; but it does not follow from this, that the system sustains more injury by the want of elimination of bile, than by its secretion and absorption. The cases usually end fatally, not because the blood is more vitiated, but because the vitiation, such as it is, arises from, and is accompanied by more serious disease. If, then we cannot account for these cases of sudden coma, by any absolute effects of retained bile, it only remains for us to inquire whether they might not be explained on the supposition of previous cerebral disease.

There are very many interesting facts, which would tend to shew, that the brain is, in some instances at least, the organ primarily in fault in jaundice. Besides the well known occurrence of abcesses, and other diseased states of the liver from injuries of the head, sudden yellowness of the whole person has not unfrequently followed intense mental emotion, and has often been observed in fevers and other diseases, in which the brain and nervous system have been much affected. Some of the cases published by Dr. Marsh, to which I have already adverted, seem to have depended on an affection of liver, and of the mucous coat of the intestines, originating in cerebral disease. If, in such instances, we could suppose the affection of head to be so obscure as altogether to escape the attention of the practitioner, previous to the occurrence of jaundice, there would be little or no indication of an unusually dangerous form of the disease. He would almost necessarily attribute the headach, languor, and sickness of stomach to the retention of bile in the circulation, and the supervention of coma and apoplexy would seem sudden and unaccountable; when, if he could have suspected the source of the disease, it would have been anticipated as a very probable termination.

We have not, unfortunately, a sufficient number of reports of

post mortem examinations in those cases, to form any decided opinion on the subject. If, with such imperfect materials, even a conjecture might be hazarded, I should, on the whole, be disposed to say, that the cerebral affection is rarely the primary disease, but is superinduced, we know not how, by the suppression of a most important excretion, as it sometimes is in the suppression of the catemenia, and almost always of the urine.—When we find inflammation of the brain or its membranes suddenly brought on by the obstruction of the uterine or renal discharges, we cannot be surprised that a suppression of one of the most important in the whole system, whether as a secretion or an excretion, should occasionally induce it. That the occurrence of coma in jaundice generally depends upon some such state, suddenly induced, and not upon previous or long standing cerebral disease would seem very probable, from the success of the treatment, and rapid recovery of two of the cases reported, and from the fact, that in the only one of Dr. M'Leod's in which there was an examination after death, no disorganization of the brain was discovered. I cannot tell why cerebral inflammation should arise from obstruction to the flow of bile in one instance, and occasion no such result in a hundred others, any more than I can account for its somewhat rare occurrence in suppression of the menses. These observations are offered, however, not with a view to the solution of the difficulties which have been pointed out, but as a faint light to the practitioner, until the question can be investigated with some prospect of success.

PROBLEM VII.

BY D. GRIFFIN, M. D.

IS THE LAW OF VISIBLE DIRECTION, AS AT PRESENT RECEIVED, A TRUE ONE?

The controversy as to the cause why an inverted image on the retina gives us a perception of the object looked at in its erect and natural position, has, it is well known, been terminated by the discovery of certain properties in the retina itself, which were said to be resolvable into a remarkable law, that afforded a perfect explanation not only of this, but of many other circumstances regarding vision. These properties were believed to be on original endowment of that membrane, and impressed on it for the purpose of connecting our perception of the objects that surround us with their real positions, directions, and other qualities appreciable by sight, in a uniform and simple manner. This law is expressed by saying, that "whenever rays proceeding from any point of an object are brought to a point on the retina, the whole of these rays, or any of them, will represent that point of the object in the direction of a line perpendicular to the part of the retina on which they fall, without any regard to the degree of obliquity with which they may have fallen on it. It had its origin in the observation of the following circumstances, which I give particularly, that, in denying its existence, I may not be thought to underrate their importance:—

1. If we make moderate pressure with a blunt point on the ball of the eye, a black spot, surrounded in most cases by a luminous ring, will be seen; and this spot will take up a position opposite to the point impressed.

2. As the pencil of rays by which any point of an object is seen has its greatest breadth at the cornea, converging from thence in two cones towards the object and the image on the retina, we might expect that, by shutting out all of the pencil except a few rays which pass in near the margin of the pupil, the object would seem to shift its place, since in this case we see it by means of rays which come to us by a circuitous course, and of which no portion that pierces the eye points directly from the part of the retina impressed, towards the object. We shall find, however under these circumstances, that the object keeps its position, and is seen in its real direction as perfectly as if we had admitted the whole pencil.

3. In following the course of a pencil of light through the eye, it will be found that pencils entering in angles of 45° and upwards from the axis, do not contain a single ray which points directly from the part of the retina impressed towards the object, yet it is well known that objects at such angles, though indistinct from other causes, are seen in their true directions.

4. Another argument in favour of this property may be drawn from considering the situation of the punctum cæcum, or blind point of the retina. Objects below a certain size became invisible some distance to the right of the axis in the right eye, and to the left of the axis in the left; whereas the entrance of the optic nerve, on which this defect depends, takes place to the left of the axis in the first case, and to the right of it in the second.

Other facts are mentioned of the same nature and tendency; but the most remarkable, and—simple and beautiful as it is—by far the most important of all, in confirmation of the existence of this property, is the following experiment of Scheiner:—

If we take a small object, the head of a pin, for example, and bring it before the eye some distance within the best point for distinct vision, we shall see it enlarged, but hazy and indistinct, the rays from it being too divergent to be brought to a point on the retina. If we now place a card, perforated by a small pin-hole, between our eye and the object, a portion of that divergent pencil will pass through the hole, the rays of which are so nearly parallel that they will be refracted to a point. We shall now, therefore, see the head of the pin magnified and perfectly distinct; but the remarkable circumstance is, that, keeping the pin steady, if we move the card up or down, or to the right or

left, which movements, of course, change the place of the image on the retina, the object will appear to move in an opposite direction in each case, though it shall not actually have changed its place at all.

Thus the first-mentioned facts prove that obliquity of incidence in the rays does not effect any apparent change in the place of the object, provided they still fall on the same point; while Scheiner's experiment proves that when this point is varied, there *is* an apparent change of place, and that this apparent change is in a direction opposite to that in which the image on the retina is made to move.

An interesting variation of the experiment, and one which leads to the same conclusion, is the following:—If we make two additional pin-holes in the card, one at each side of the former, and just so near it that the rays from the pin head through the whole three may pass through the pupil together, by holding it between our eye and the object, we shall now see, as it were, three pins' heads. If any one of the holes is stopped, one of these will disappear; and by trials we shall find that, when the hole at the right side is stopped, it is the left pin that disappears, and *vice versa.*

I have been thus particular in stating the law as at present received, with some of the principal facts on which it rests, because I think some circumstances have been overlooked which are utterly incompatible with its existence. A few years ago, being engaged in delivering some public lectures of the most elementary kind, on optics, at the Limerick Institution, it fell to my lot, during part of the course, to describe the structure of the eye, and its action as an optical instrument. In drawing the necessary diagrams, some cicumstances occurred to me, which impressed me strongly with the conviction that this law could not be true. As, however, any discussion on the question would be out of place before an audience, many of whom were not familiar with the subject, I gave the explanation which is now generally received—a very unpleasant task to execute while I had the feeling of its fallacy so strong upon me. My attention has been drawn to the subject from time to time since; and though during that period I have seen many writers receive it as if it was well established, yet of this at least I am now certain, that the above law cannot be true under the conditions specified.—These con-

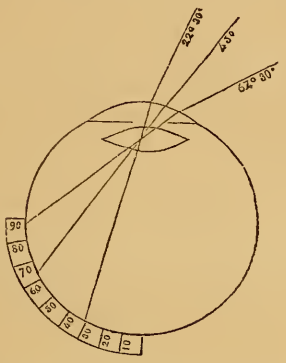
ditions are, that the concave surface on which the retina is expanded is spherical, or nearly so, and that the curvatures of the different humours of the eye, and their indices of refraction, as given by the best authorities, do not differ widely from the truth.

The first of these conditions has, I believe, never been disputed; with regard to the second, the refractive indices and curvatures of the different humours have been examined by M. Petit, Drs. Young, Brewster, Gordon, Wollaston, and others; the following are the results; such of them, at least, as are necessary for our present purpose :—

Interior transverse diameter of the eye	90
Horizontal cord of the cornea	·49
Radius of external surface of cornea	·33
Radius of anterior surface of lens	·33
Radius of posterior surface of do.	·24
Distance of iris from cornea,	·11
Distance of lens from do.	·12
Refractive index of aquaous humour	1·337
Refractive index of lens (mean refraction)	1·384
Refractive index of vitreous humour	1·339

Let us suppose the interior of the eye, on which the retina is spread, to be graduated from 0 to 90°, and so on, beginning from the point where the visual axis strikes that membrane, and marking this point zero. If now, using the above curvatures and refractive powers, the progress of a pencil of light be traced graphically, following the usual law of the sines, it follows that if the above law of "visible direction," as it has been called, is true, the number of degrees marked on the point of the retina where it is found to fall, ought to indicate the degree of inclination of the pencil to the optic axis before it entered the eye; that is, in fact, ought to indicate the actual direction, in space, of the object from which it came, or at least, according to Sir D. Brewster's version of the law, ought to indicate the position of a line parrellel to it, which would come to the same thing. In tracing the ray thus, the mean refractive index of the crystalline may be taken, instead of its varying one, without affecting the course of the pencil in any degree worth speaking of. Indeed, it will be found that by far the greatest portion of the whole refraction that takes place in the eye is produced at the cornea, and that when this is accomplished, the refractions produced by

the lens and vitreous humour have but little influence further on the direction of the ray. The accompanying figure is a copy of



a much larger one, drawn in conformity with these conditions, and it will found that, tracing the course of the ray in the manner described, a pencil inclined 22° 30' to the visual axis, will fall somewhere about 34° on the retina; one inclined 45°, will fall on a point marked 63°; and a pencil inclined 67° 30', will fall on a portion of the retina which, if it possessed the property above mentioned, must represent it as standing nearly 89° from the axis of vision. Pencils at intermediate stations will be found to deviate in intermediate degrees; but in every case there is an error, which increases with the inclination of the pencil, though not exactly in the same ratio.

Now it is well known that exteriorly the field of view extends even 90° from the axis—that is—when the eye is directed straight forward, we can see objects in any position, from 0 up to 90°, on the outside of the eye, and see them in their true positions. But it will be found, by trials on a diagram such as I have described, following the same rule as for the other rays, that a pencil of light from an object removed 90° from the axis, cannot possibly be brought to a point marked 90° on the retina. The question is not here, whether we can see them distinctly or not, but whether we can become sensible of their presence, and if they are represented in their true positions; and neither of these points, I believe, has ever been disputed.

Thus it is evident that this law, stated, as it has been, on what I cannot help calling a very loose examination of the circumstan-

ces, is not true, nor even nearly true; and we do not get out of the difficulty by supposing that the indices of refraction of the different humours have been under-rated; for (not to speak of the improbability that such a large error could be committed in ascertaining them as this supposition would require) it is clear that the refractive power necessary for bringing pencils entering the eye at angles of 45°, or 70°, to points at the same angular distance from the extremities of the visual axis, would make the eye myopic for pencils lying near the axis, and would thus render the very best part of the retina utterly useless. We cannot, therefore, put the facts we have collected under any general expression, but must rest satisfied, for the present, with the simple statement, that when rays of light fall on any point of the retina, that point has the property of representing the object from which they come *in its true direction*, without any regard to the obliquity of their incidence.

I have examined, with the assistance of another person, the positions of the spectra when pressure is made on different parts of the ball of the eye, in order to see how far facts of this kind support the view which is sought to be maintained. Extreme accuracy cannot be expected in such examinations, but the following are the results, as near as possible.—When the pressure is made 90° from the axis on the outside, the spectrum appears anterior to the bridge of the nose; when the axis is directed towards this latter part, and pressure is made as deep as possible on the outside of the eye, the spectrum appears a little within the bridge of the nose; when the axis of the eye is directed outwards as much as possible, and pressure is made as deep as one can at the inner canthus, the spectrum stands about 30° on the outside of the point to which the axis is directed; and generally speaking, I find that whatever the position of the axis when the pressure is made round the ball of the eye and within the edge of the orbit, the spectra usually appear round the margin of the field of view. I noticed a curious circumstance in making these examinations, which is, that though motion of the blunt point on the eyeball produces very free motions of the spectrum, yet, when pressure is made on any point, as the inner canthus, for instance, so as to produce a spectrum, and the point is then kept perfectly motionless, and the axis of the eye is directed inwards or outwards, which one would suppose amounted to the same thing

as movement of the point itself, in this case the spectrum remains almost perfectly at rest. If the axis of the eye is directed up or down, in the same circumstances, there *is* a movement of the spectrum, which, however, is not at all proportional to the movement of the axis, Though these facts may seem incapable of explanation, and even inconsistent with each other, they evidently give no support to the notion that the retina has the property of representing objects in lines perpendicular to its surface.

Assuming this law of visible direction as true, Sir D. Brewster endeavours to account for the stability of objects which occupy the field of vision during the motions of the eye, by supposing that the centre of visible direction, or the point through which all the lines of visible direction pass, is coincident with the centre of motion of the eye. He says (Treatise on Optics, p. 224, "when we move the eyeball, by means of its own muscles, through its whole range of 110°, every point of an object within the area of the visible field, either of distinct or indistinct vision, remains absolutely fixed; and this arises from the immobility of the centre of visible direction, and consequently of the lines of visible direction joining that centre and every point in the visible field." That objects within the visible field remain fixed during motions eye, is a fact; but that the above is the cause of their stability, is by no means true. He says further, "Had the centre of visible direction been out of the centre of the eyeball, this perfect stability of vision could not have occurred." It certainly could not, if the centre of motion coincided with the centre of the eye, and that the retina had the property stated; but the stability of objects, as I shall presently shew, is perfectly compatible with other properties of the retina: in fact, the supposition of the existence of the property stated, is the only thing that could make a difficulty about it. He says further (same page,) "If we press the eye with the finger, we alter the spherical form of the surface of the retina; we consequently alter the direction of lines perpendicular to it, and also the centre where these lines meet, so that the direction of visible objects should be changed by pressure, as we find them to be." Now this is a complete misapprehension of a fact; the directions of visible objects are changed, in this instance, not from any change in the form of the eye, but because it is either directly pushed or

drawn out of its direction towards the object, by actions induced in some of its muscles by the pressure; for precisely the same change of situation will be produced, even more perfectly, by laying hold of the under or upper eyelid, and drawing the eye in different directions by means of its attachment to the conjunctiva; a proceeding which cannot give rise to any alteration in its sphericity.

The following observations will lead, as it appears to me, if not to clearer and more simple, at least to truer notions on the subject of vision, than those usually received:—In the first place, as the refraction of the humours of the eye is unchangeable, it follows that rays standing at the same angle from the axis will always be refracted to the same part of the retina; they will not one time come to a certain point, and another time be bent more deeply into the eye, but will always strike the membrane at the same distance from the extremity of the visual axis. Considering, therefore, the nervous matter of the retina as made up of numerous zones distributed in parrellel bands around the point where the visual axis strikes the back of the eye, we see that rays entering the eye at an angle of 45° for instance, with the axis, will fall on a zone of nervous matter situated somewhere about 63° from this point. Rays from every object standing at that angle from the axis all round, must fall upon some part of this zone; no rays from objects standing at other angles can ever touch it; and we find that this zone has the property, when rays fall on it, no matter with what obliquity, of representing the object from which they come as standing 45° from the axis. It must be remembered that I am now speaking of facts: that the retina has this property cannot be disputed, however it may be supposed to have come by it; whether it was an original endowment of the membrane, or an acquired property, is another question, one which seems to me to have lost its interest, and not to be of much importance: yet I would remark, that the circumstance of our being at length obliged, as I feel we are, to resolve all our knowledge of vision, not into a general law but into a simple statement of the fact, that all parts of the retina have the property, no matter with what obliquity the rays fall on them, of representing the objects from which they come in their true directions, is a strong argument for the latter supposition; for this is precisely what we should expect to occur as the result of

experience. Taking each of the other zones of the retina in the same manner, we see that our perception of the angular distance of every object from the axis is predetermined by the zone of nervous matter on which rays from it fall, and its actual position by the part of the zone to which they are brought. Without speculating on the cause of these properties at all, it is evident that under this arrangement there can be no instability of objects in the field of view during motion, wherever we suppose the centre of motion to be situated, and whatever point the centre of visible direction, if any such there be, may occupy. In fact, as I have said before, the conferring on the retina a property of representing all objects in the direction of lines perpendicular to the surface on which the rays impinge, is the only thing that could endanger their stability during motions of the eye, since, in this case, the coincidence of the centre of visible direction with the centre of motion and centre of curvature, would be absolutely essential to its maintenance, and which conditions would be by no means necessary in any other case. A little consideration will show this to be a fact.

There is something so attractive in the simplicity with which a general law groups and classifies various kinds of facts, that we are apt to be dazzled and caught by it, and to overlook difficulties which if traced to their consequences, would certainly compel us to its rejection. I cannot otherwise account for the circumstances that a passage in the fifth edition of the late Dr. Turner's Chemistry, which I fell upon while writing this paper, should be so entirely disregarded by Sir David Brewster, Mr. Mayo, and many other physiologists, who, notwithstanding its clearness, still adhere to the explanation I have been endeavouring to refute, and which seems so wholly untenable. In that passage Dr. Turner expresses himself not satisfied with Sir D. Brewster's statements as to the law of visible direction, and traces its operation to consequences very analogous to those which I have exhibited above. I cannot help expressing some surprise that consequences so obvious as these should have escaped so many ; the drawing even of a single diagram of the eye, if the circumstances are at all attended to, would inevitably lead to them, for it will be found in every case except where the object is in the axis of the vision, that the law usually adopted would represent it out of its true position, and the line of visible direction will not even, as Sir D.

Brewster says, be parrellel to the line of its true position. Even when situated in the axis, it will, if it is of any size, be magnified and distorted. Mr. Mayo, indeed, seems to have met with some difficulty of this kind in drawing the diagrams for the fourth edition of his Physiology (1837); for finding, on trial, that lines drawn from the point of the retina on which the rays fell, towards the extremities of the object, were not perpendicular to the retina, he very strangely, instead of questioning the truth of the law, seems to be obliged to modify his expressions regarding these "lines of visible direction," and says, in proceeding from a consideration of the simple lens and luminous point, to a diagram of the eye—"Let me substitute for this diagram one of the eye in vision, in which the dotted lines, A B, C D, are *meant* to be vertical to the points A and C, and *are to be understood* to be so." It seems curious that so accurate an observer as Mr. Mayo usually is, should not have followed this difficulty into all its consequences. Had he done so he must necessarily have arrived at the same conviction that Dr. Turner expressed so clearly, nearly four years since, and that I have been endeavouring to establish in this paper.

From the slight difference in refractive power that exists between the aqueous, crystalline, and vitreous humours, the influence of the two last on a ray of light is very trifling, so that, as I have mentioned above, by far the greater part of the whole refraction takes place at the cornea. Though this is pretty generally known, I am afraid the small amount of the refraction is scarcely sufficiently considered. Indeed, when once this first refraction has been effected, the course of the ray afterwards is so nearly that of a straight line, that a superficial observation would lead one to think that no allowance had been made for the remaining refractions. A most important practical inference arises from this; that is, the immense importance of preserving the original shape of the cornea in operations for the extraction of cataract. Those who are familiar with the grinding of lenses and specula, and who know what exceedingly minute errors of figure are capable not only of giving rise to indistinctness, but of destroying all distinctness in the image, will percieve the importance of this remark. The loss of the lens must not be considered the greatest injury the eye has sustained in this case; indeed, I strongly suspect that the uncorrected spherical aber-

ration of the cornea is the principal source of the indistinctness that remains even with the use of glasses, after those operations for cataract, even of the most successful kind, in which the cornea has been untouched. The aberration of lenses being imperceptible in small refractions, and the refraction of the lens being exceedingly slight, in consequence of the slight difference in refractive power between it and the aqueous humour, it can scarcely be supposed, if the index of refraction given be at all near the truth, that its peculiar structure of diminishing density was introduced for the purpose of correcting its own aberration, which could never be worth taking into account.

I have never seen, nor even heard, of an operation for cataract, in which perfect distinctness was produced; yet this ought sometimes to sake place in operations for soft cataract, if the cornea contained a means of correcting its own aberration, and the only injury the eye had sustained was its being made somewhat presbyopic from the loss of the lens; for a glass lens would, in such a case, make up for the deficiency of refraction, without introducing any perceptible aberration. It is therefore of great importance not to introduce any incorrigible, or unsymmetrical aberration, by any carelessness about the shape of the cornea in the management after the operation. The great indistinctness which such unsymmetrical refraction may occasion in an eye otherwise good, is fully shown by Professor Airy's observations on the defective figure of the cornea of his own eye, the discovery of the nature of which, and the success of the remedy applied, form one of the most interesting instances on record of the application of optical knowledge to the removal of imperfect vision. I do not make these remarks from any apprehension that the replacing of the flap after extraction, or the accurate healing of the wound, could ever be considered matters of light moment, but simply from this circumstance, that when an end is sought to be attained, it is always of consequence to hold clearly in view the most important principle upon which its attainment is desirable.

While engaged in writing the above, Professor Rainy, of Glasgow, directed my attention to a paper on the same subject which appeared in the ninth number of Poggendorf's *Annalen*, by Professor Mile. I understand that he also denies the existence of the law I have been discussing; but I have not yet been able

to procure an account of his facts or reasonings on it, which I am very anxious to do.

I have seen several articles in the medical periodicals, many of them containing very ingenious speculations on single and double vision, and other subjects relating to optical physiology, by Mr. T. Williams, Mr. Grove Berry, Dr. Graves, of Dublin, and others. All these gentlemen, as far as I can perceive, found their reasonings on these subjects on the supposition of the truth of Sir D. Brewster's law of visible direction; most of their opinions, therefore, do not admit of discussion while this remains unsettled. If they doubt the assertions I have made above, with regard to the refraction of the rays, all I have to request is, that they will take the trouble to draw a large diagram of the eye, using the refractions and curvatures given above, and inform me what part of the retina rays at the angles above mentioned really do fall on. This is the great and fundamental point, and we cannot proceed a single step until it has been fully determined. I do not contend that my determinations of these points are perfectly accurate, but I have no hesitation in saying, that they do not contain any error which, if removed, would make Sir David Brewster's law at all possible. Dr. Graves, in one of the numbers of the Dublin Journal of Science, in speaking of this law, expresses some surprise that the Rev. H. Lloyd, and Sir J. Herschel, should, in their works, have taken so little notice of it. I confess I was myself surprised at their being so little influenced by views which were stated so universally, and seemed to be supported by various facts; but though I still think many of their views regarding vision incorrect, a particular examination of the subject has made me feel the necessity of every law being based upon principles that cannot be shaken, before one can insist on its universal reception.

Before proceeding to another part of the subject of vision, I must mention a curious fact regarding the intolerance of light, which I discovered some time ago. If we look at a bright sunny road in the height of summer, or at one of those white fleecy clouds called cumuli, the light is so intense, that besides the pupils being contracted to the utmost, we are obliged to cover a considerable portion of them by half closing the lids. In these circumstances the sensation of intolerance is felt in the eye, and may be thought to have its seat in the retina. If, however, we

close one eye entirely, we shall find that the other may be then freely opened without uneasiness, which shews that the real seat of the sensation must be some part of the sensorium itself, and not the retina, which is actually then receiving more light than before. We have here, therefore, a highly intellectual sense—intellectual as regards its anatomical connexion with portions of the brain devoted to the process of thought, and intellectual as regards the mental processes which many of its perceptions imply; exhibiting, at least as far as concerns its common sensibility to light, the same law which has been found to prevail in other parts of the nervous system—namely, that when a certain state is induced at the centre of the nervous mass, the resulting sensation is referred to its extremity. This curious fact may, perhaps, be of some importance in the management of those annoying and intractable forms of ophthalmia, in which intolerance of light is so prominent a symptom.

The following experiments were undertaken, for the purpose of determining the situation and size of the punctum cæcum of the retina. The greater number of them were performed in the following manner:—The back of the head being placed in contact with one wall of the apartment, the distance was measured, as near as possible, from the centre of the eye to the opposite wall. A candle was so placed as to make its image appear in the centre of a convex mirror hung there, which gave the flame of the candle a small and star-like appearance, better adapted to the experiment. The right eye being then fixed first on the image, was directed to the left of it, and at the last point, where I was certain I could see it, a wafer was placed on the wall. Moving the eye still to the left, a wafer was placed again on the wall, at the first point, where I was certain I could *not* see it; going on still to the left, a wafer was placed at the last point, where I was certain I could *not* see it, and again at the first, where I was certain I *could*. Drawing a line now, from half the distance between the inner wafers to half the distance between the outer, it is evident that this line might be taken to represent the angular breadth of the insensible spot; and, accordingly, when the right eye was directed to the middle point of this line, the image of the candle was perfectly invisible, from its then falling on the centre of the blind spot. Moving the eye upwards and downwards from the middle of this line, the vertical diameter of the

spot was obtained in the same manner. The length of these diameters being measured, as well as the distance from the centre of the mirror to the point where they crossed, the lengths thus obtained were divided by the distance of the centre of the eye, which gave the tangents of the angle subtended by the blind spot, and of its angular distance from the visual axis. As it is not necessary to go into these calculations, I have just subjoined the results. That I might not be misled in repetitions of the experiment, by my having taken the measures of those that I first performed, the experiments were varied in different ways as to distance and light; and with the same view, the calculations were not entered upon for any until the whole were completed.

FIRST SET (RIGHT EYE.)				SECOND SET (LEFT EYE.)			
No. of Experiments.	Distance from Visual Axis	Diameter.	Centre of spot above Visual Axis.	No. of Experiments.	Distance from Visual Axis.	Diameter	Centre of spot above Visual Axis.
1	15° 4′	7° 11′	1° 10′	1	15° 24′	7° 31′	1° 47′
2	15° 44	7° 31′	(1° 18′)	2	15° 28′	7° 31′	(1° 18′)
3	15° 6′	6° 59′	1° 29′	3	15° 50′	6° 19′	1° 37′
4	16° 6′	6° 12′	(1° 5′	4	15° 49′	6° 19′	1° 38′
5	15° 11′	3° 15′		5	16° 18′	6° 0′	(1° 5′)
				6	15° 30′	2° 45′	
Means	15° 26′		1° 15′	Means.	15° 43′		1° 29′

Taking the means, we have, therefore, for the distance of the spot from the visual axis in the right eye, 15° 26′, and in the left 15° 43′. Taking the mean of these means, we have 15° 34′ as the most probable value from these experiments. But it would not be fair to take a mean of the diameters of the blind spot in the same manner, for the differences which the tables shew seem to depend upon circumstances which I will now explain and which lead to a conclusion of some importance. Some of the experiments were performed in the way I have just described; some by placing a circular paper, seven or eight inches in diameter, on a light-coloured wall, and standing just so near it that the whole would be completely but barely hidden, when the axis of the eye was turned in a proper direction; others, again, were

performed by shading the flame of a candle with a cylinder of dark paper, in which a small hole was cut for its light to appear; the experiment, in other respects, being proceeded with as at first described. Lastly, they were done in Dr. Young's manner, with two unshaded candles. Taking, therefore, for each eye a mean of those experiments in which all the circumstances were alike, and distinguishing the others, we have the following interesting results:

RIGHT EYE.		LEFT EYE.	
Nature of Experiment.	Diameter of Spot.	Nature of Experiment.	Diameter of Spot.
With paper on light coloured wall	7° 31′	With paper on light coloured wall	7° 31′
With image in mirror	7° 5′	With image in mirror.....	6° 19′
With luminous point..... through the cylinder....	6° 12′	With luminous point..... through the cylinder....	6° 0′
With unshaded candles....	3° 15′	With unshaded candles....	2° 45′

Here we have first a white paper on a light-coloured wall, in which the light is feeble and the contrast slight, we have, therefore, a large diameter. Next we see the image in a convex mirror, in which more than half the light is dispersed and lost; there is still, therefore, a tolerable large diameter. Next we have the direct light of the candle seen through a small aperture, by which the intensity of the light is twice as great as before, and we have a diminished diameter; and lastly, we have unshaded candles (Dr. Young's method,) in which, besides having light of the same intensity, we have a considerably increased quantity of it; and in this case the diameter is less than in any other. Hence we see that the diameter of the spot is diminished as the strength of the light increases; and this circumstance seems to indicate at once the cause of the blindness which appears to owe its origin, not as Mayo and others suppose, to the presence of the artery in the centre of this spot, but to the thickness of the nervous matter at this part; the optic nerve not having yet spread out into those thin filaments which are exhibited in the structure of the other parts of the retina. This conclusion best explains the facts; for we see, that at some distance

from the centre of the optic nerve, its sensibility seems dull to moderate lights, and it is only capable of being roused by very strong lights at the centre itself. Indeed I found the centre perfectly insensible to the image in the mirror, when it was directly brought opposite it. The optic nerves, in this respect, resemble other nerves in the body which are not fit for their functions until they have been distributed in thin and fine filaments. Moreover this conclusion, if true, seems to be important in another point of view; for if the thickness of the nervous matter here is the cause of the blindness, it may reasonably be asked whether thinning down of the nervous matter which takes place, according to some anatomists, around the so called foramen of Soemmering, may not be the cause of the great acuteness of vision that exists at the end of optic axis?—supposing it should be found, by experiments of the above kind and by measurements of the eye, that these two parts coincide. I found the presence of the artery at the centre of the optic nerve quite perceptible by a reddish glare, which shewed itself about the centre of the invisible part of the field; but this appearance only took place in the experiment with unshaded candles.

In performing the above experiments, I found a tendency in the eye, in moving outwards, to move also a little upwards; from the circumstance that the image, or candle, was more perfectly hidden then, than when the axis of the eye was directed to a point in the same horizontal line with it. This seemed to indicate that the punctum cæcum was situated a little higher than the extremity of the visual axis. It is evident, however, that no complete proof of this could be obtained, except from experiments performed with both eyes at the same time, since there is otherwise nothing to assure us that the head is not placed obliquely during the experiment; which would not much matter in the case of both eyes, as one would be depressed as much as the other was elevated, and we could take a mean. The results of the experiments performed with both eyes at the same time are given in the third column, and included in brackets. They represent the centre of the optic nerve as elevated above the plane passing through the visual axes of both eyes: in one experiment $1^{\circ}\ 18^{\prime}$, and in another, $1^{\circ}\ 5^{\prime}$. I say the plane containing the axes of both eyes, because it is evident that many planes might contain the axis of one eye. Taking the mean of these values, we have

1° 11′ as the probable elevation, from experiments on both eyes at the same time. I have not, however, discarded the others, as they are uniform in indicating an elevation, though it may not be, and is not probably correct in its amount.

In most of the experiments with the mirror, the vertical was somewhat longer than the horizontal diameter. I had reason to think, however, that this arose from the flame of the candle being longer in the vertical than the horizontal direction: and when I made use of the cylinder of paper, with a round hole in the side to expose a part only of the flame, this difference at once disappeared.

Experiments such as the above, if repeated carefully, would give a means of determining the part of the retina on which the visual axis rests, or whether it coincides with the symmetrical axis, or what is the amount of its deviation from it if it does not. M. Le Bat considered the diameter of the insensible spot to be about one-third or one-fourth of a line. Daniel Bernouilli found it to be about one-seventh part of the diameter of the eye, and Dr. Young made it about 5°; while many experiments, performed in the same manner with candles, represent it as about 3°. Such differences are of less importance when considered with reference to the varying circumstances mentioned above, on which they depend. I dare say a small point of light, of exceeding intensity, would assign a very small diameter to the insensible portion of the retina, if it was capable of discovering it at all.

PROBLEM VIII.

BY WILLIAM GRIFFIN, M. D.

IS LARYNGISMUS STRIDULUS, OR THE CROWING DISEASE, A SPASMODIC OR PARALYTIC AFFECTION?

BEFORE entering on the discussion proposed in the foregoing problem which has been suggested by the ingenious and clever work of the late Dr. Ley, it may be of use to give a sketch of the few casess of laryngismus stridulus which first attracted my attention to the subject.

The complaint does not appear to be so remarkably rare of occurrence, as was imagined when it first came to be accurately described by medical writers. The cases I shall relate all happened within the last two years in my own practice, and the great majority of the profession who have written on the diseases of infants latterly, evince a familiar acquaintance with it. The fact that an affection, I might almost say so common, should have been completely overlooked by almost all the eminent men of past times is sufficiently mortifying, whether looked upon as illustrative of the difficulty of the diagnosis, or a general inaccuracy of observation.

I shall first describe two cases in which the phenomena of the crowing disease formed but a minor or less important part of the whole affection. They are curious as illustrations of a complaint which I believe has not even yet been noticed by any medical writer, and which might, perhaps, to distinguish it from the crowing disease, be appropriately called the *crowing apoplexy of infants.*

An infant of rather a spare and puny frame, although healthy looking in its countenance, on the eleventh day after birth, was

affected with bowel complaint. The motions were very fluid; of a light yellow colour, and there were about six in the course of the day. It got a chalk powder from the nurse at night, and next morning a drop of laudanum. The motions were less frequent, though still loose, and the child did not look well. It had been fed on milk and water, and a little prepared barley from its birth, the flatness of its mother's nipples preventing its obtaining much nourishment at the breast; and it was now suckled by a woman whose child was twelve month's old, but perfectly healthy, while they were waiting to procure a younger nurse. In the evening, however, it was seized suddenly, as if with suffocation, losing its breath, and becoming first pallid, afterwards dark or purplish in the face, and finally, when all respiration was suspended, of a death-like hue. There was a stiffening of the frame and twitching about the mouth, and the thumbs were drawn into the palms of the hands; but there was no convulsion of the body or extremities. After a longer or shorter interval the breath was recovered by gasps, which were accompanied by a crowing sound, that became louder after a time so as sometimes nearly to resemble hiccup. This crowing again gradually died away, and the respiration became easier, but always recurred on the approach of another fit, and continued until the breath was lost, when the child, as in the first instance grew dark for a moment or two, and then pale and death-like. It was indeed sometimes impossible in these intervals of suspended respiration, which recurred frequently, to say whether th child was living or dead. It lay cold, white, breathless, and without sign of animation, often for three or nearly four minutes counted by a watch, and then recovered with a faint gasp, followed by the crowing. The intervals between the fits of suspended respiration were seldom longer than half an hour, although they occasionally extended to an hour or more. The crowing sometimes continued throughout the interval, at others abated for a little. From the first moment of the attack the infant never recovered its consciousness; the pupils seemed fixed, the eye senseless, and all power of deglutition was lost. Whenever a teaspoonful of liquid was given, it remained in the mouth or flowed out at the corners, or if it went back to the larynx it obstructed the breathing, and brought on the fit of suspended respiration. The only approach to sensibility observable at any

time was in the slight motion of the lips, which were sometimes seen to work as in the action of sucking. There was rarely when the fit of suffocation commenced, a slight convulsion or twitching of the muscles of the face, which however, never lasted longer than the blackness or darkness, and seemed in fact a struggling for breath; but as the case became protracted, although the crowing in the intervals continued as loud as before, no darkening of the face or writhing of the features preceded the fit.

As the power of swallowing was gone, the treatment was confined to injections, stimulating liniments to the spine and stomach, and the warm bath. The first consisted of starch and assafœtida. Turpentine was afterwards used, and when these means seemed unavailing, laudanum was administered. The injections were sometimes retained for half an hour, but usually came away soon, and in no case had any perceptible influence in preventing the fits, although as much as eight drops of laudanum were given in this way. The bath at first seemed of service, and prolonged the interval, but after some repetitions lost its effect. A blister, which was applied to the back of the neck, was equally fruitless, and after an illness of about forty hours' duration, the poor little sufferer ceased to breathe any more.

CASE II.—A fine, round-limbed, healthy-looking boy was born after a favourable labour of about six hours' duration. He got castor oil, as infants usually do, and on the morning after, I was requested by the nurse to inspect the motions, which were very green; the one she showed me had the appearance of chopped spinach. I directed a grain of calomel to be given, and if it did not move the bowels freely, the oil in the morning. Next day the motions still continuing green, though less so than before, the calomel was repeated. On the fourth day the bowels were much better, and on the fifth the evacuations seemed to be of the ordinary character. The child during this time got nothing except the breast, and (the mother not being able to nurse long nor often on account of sore nipples) a little prepared barley, or milk and water. It appeared to thrive, and did not lose its plumpness, but the mother afterwards informed me it used to start frequently with a cry or scream out of its sleep in a way she had never observed with her first child, who was alive and well. After this sudden start or scream it usually feel asleep,

immediately. On the evening of the tenth day, when my attendance had terminated, the child was attacked with slight complaint in the bowels, for which it got a little chalk mixture. The bowels were moved five or six times during the night, not very profusely, and the evacuations were of a pale yellow colour. At eight o'clock the next morning I was summoned hastily to visit it, and on entering the room, to my utter consternation, I found it gasping, after a fit of breathlessness precisely similar to that described in the former case, and followed by the same crowing noise in inspiration. It was perfeetly insensible ; the pupils of the eyes were natural in their appearance, but sluggish in their movements ; the power of swallowing was gone ; the thumbs were bent into the palms of the hands ; the surface was pale and cold ; the impulse of the heart and the pulse at the wrist were feeble. The fit of suspended respiration occurred at intervals of perhaps twenty minutes, though they were sometimes longer. The child's countenance did not darken in the fit as in the other case ; it became instantly pale on losing its breath ; the lips and even the tongue were cold, and when I put back my finger to the pharynx, to ascertain whether I could by the touch excite the action of swallowing, I found the parts motionless. The crowing continued almost through the whole of the interval, and if it ceased, it always recurred on the approach of the fit. On one occasion, after the warm bath, it seemed to subside into a breathing slightly stertorous, which did not at all occur in the case of the former child. There was, however, once or twice in the course of the day the same working or sucking motion of the lips which I noticed in that case, indicating some faint approach to sensibility.

The treatment of this little patient differed in no respect from that employed for the former infant, except that no laudanum was administered, and it was equally unsuccessful. If it could be said that anything gave the least relief, it was the warm bath. A blister to the nape of the neck and vertex was applied early, but before it could have had any effect the little sufferer expired, having struggled altogether only six hours against the disease.

On examining the body of the first infant no appearance of disease was observable in the heart, lungs, or bowels. On opening the head there was excessive difficulty in detaching

the skull from the dura mater, and as soon as it was removed the hemispheres fell asunder in a diffluent or pultaceous mass, so that it was impossible to make any regular examination.—Having removed the gelatinous mass of brain and cerebellum, the medulla oblongata and spinal cord were found of healthy consistence.

I now thought I had a clue to the explanation of the symptoms. There appeared to be here sufficient disorganization to account at least for the unconsciousness and attach probability to the supposition that the crowing disease was dependent on some affection of the brain. Billard speaks of such general ramollissement, as often occurring immediately after birth, and mentions that it is then more considerable and extensive than at any other period of life. He thinks it probable that it sometimes begins even before birth. I do not know what the attending symptoms were, but in ten such instances in which the softening extended to the whole of the spinal cord, he relates, that the respiration was laborious and imperfect, the limbs flaccid and motionless, and the pulsations of the heart scarcely perceptible. These symptoms were all absent in the case of the infant which I have detailed, it would seem because there was no lesion or disorganization of the spinal cord or medulla oblongata; but the functions of the hemispheres and of the cerebellum were altogether suspended, as the ramollissement of these parts would lead one to expect.

There could, however, be no stronger proof, that identity of functional derangement is no evidence of the identity of the lesion which produces it, than appeared on examining the body of the second infant, the symptoms of whose disease so closely resembled those of the first. The brain seemed perfectly healthy, and there was no sign of disease in any other organ that I could detect. The examination in either case was a very hurried one which precluded any examination of the state of the eighth pair of nerves, or recurrents, or of the bronchial glands. Indeed it did not occur to me as a matter of any importance to make the examination at the time, as I thought it very improbable there could be any scrofulous enlargement of these parts immediately after birth. There certainly was no perceptible enlargement of the glaudulæ concatinatæ, or other glands in the neck. The thoracic and abdominal viscera were also in this case healthy. I do

not know, however, what importance, if any, to attach to one circumstance; the cardiac orifice of the stomach was found plugged up with a firm coagulum of milk, which retained the exact shape of the parts when it was removed.

The complaint which I have described as affecting these two infants, thongh closely allied to laryngismus stridulus, or crowing disease, is obviously very distinct in its nature, or more truly perhaps in the amount of nervous matter involved in the morbid action. Possibly the same difference may exist between them as between apoplexy and local convulsion, or, as Dr. Ley sug gests, palsy. Whatever it may be, the distinction between them is very marked. In laryngismus there is no insensibility, no crowing except immediately on recovering from the fits of breathlessness, no apparent illness whatsoever in the intervals, no pallor or coldness of the surface, or feebleness of the action of the heart, or of the pulse at the wrist. In the affections which I have just described on the other hand, there was, from the moment of seizure, an utter unconsciousness and insensibility both in the fit and interval, so much so indeed, that even a cry did not escape either of the infants except in one instance, when the first was plunged into a hot bath. There was an incapibility of swallowing, excessive feebleness of the heart's action, and coldness of the surface of the body. In the second child, there was at one time, when the crowing subsided during a longer interval than usual, some approach to stertor in the breathing. I ought perhaps to notice one other distinction between this and the crowing disease of Dr. Clarke and others, that it did not terminate in general convulsions as the latter usually does.

It is exceedingly difficult even to speculate with any probability on the cause of this remarkable affection, or to connect it with any certain pathological condition. From its occurrence a few days after birth, at which period, Billard states, general *ramollissement* of the nervous centres is most common, I should have been disposed to connect the symptoms with some such disorganization, if, in the second case, the brain had not been found apparently healthy; and I should have attributed them to some congenital defect, which the weak appearance of the other infant might in some degree countenance, only that no suspicion of the kind could be entertained in reference to the second. It was born a strong, plump, round-limbed little fellow, in every way as

promising for long life as one could desire. It might, indeed, be said that the very earliest evacuations were unhealthy, and the sudden screams out of sleep described by the mother, indicated something wrong from the commencement; but these symptoms are of every day occurrence with other infants who go on well notwithstanding.

From a fair consideration of both cases it becomes a question of great interest, whether the complaint might not be one of mere functional derangement, as we are well assured many cases of fatal convulsions are. It was in both instances preceded by disorder of the bowels, and on the evening previous to the attack by diarrhœa. Supposing it to be a functional affection, to what cause are we to attribute it? An experienced practitioner informed me he had met with similar cases which also proved fatal, and that he believed them to depend upon retention of the meconium. In the first case narrated, however, castor oil was given to the infant immediately after birth in the usual manner, and repeated as occasion seemed to require; and in the second, calomel and castor oil were given until the evacuations assumed a natural appearance. Could the hard plug of coagulated milk, found in the cardiac orifice of the stomach of this child, by possibility produce such a frightful affection of the nervous centres?

I now proceed to the cases in which the question at issue between a portion of the profession and the late Dr. Ley is more directly involved. They accurately agree with those already published by that gentleman and others, and are given in detail only because the amount of those already upon record is far too slight to admit of safe general inferences as to their pathology or treatment. In all diseases, the treatment of which is difficult or obscure, the numerical system of induction so successfully adopted by Louis in affections of the lungs, is the only one worth our attention, and without a large number of cases this cannot be resorted to, even in the imperfect manner, which these, when given loosely by different individuals, may admit of.

A fine, stout, muscular little fellow, at the age of seven months was seized, in the nurse's arms, with sudden suspension of the breath, but after a slight struggle, and gasping attempts at inspiration, accompanied by a crowing sound, he in a few moments recovered. As the fits recurred two or three times in the course of the day, and occasioned a very just alarm to the parents, I was

requested to see the child, whom I found playful and smiling on the nurse's knee before me. From the description of the fit, as it was called, I had no doubt as to its nature. My acquaintance with it having been chiefly derived from Dr. Clarke's communications on the subject, I entertained his views of its cerebral origin, believing that in this case the brain had become affected from the irritation of teething in a naturally plethoric habit. The gums were therefore lanced, active purgatives were given; and the child, who was a great feeder, was restricted to a lighter diet. The complaint recurred silghtly for some days, when one or two teeth appearing, it ceased altogether, and for five or six weeks the child continued perfectly well. It was then, however, attacked with a more violent fit than before. As well as I can recollect (for I kept no notes of the case) I then lanced the gums again, blistered behind the ears, and directed some purgatives. The complaint recurred frequently when the little fellow laughed or cried, and sometimes he awoke out out of sleep with the gasping and crowing. He passed some days entirely without an attack; on others he had one or two. I now directed assafœtida and anti-spasmodics for him; but as there was no satisfactory amendment, the fits still returning occasionally in a very alarming manner, a consultation was proposed with a physician of eminence and experience. This gentleman took altogether a different view from mine of the disorder, and I saw evidently that he had either never seen it, or never distinguished it as a specific affection. He spoke of the crowing as singultus, and attributing it to acidity of stomach, with derangements of bowels, recommended that milk and its other usual diet should be laid aside, and broth substituted; a carminative mixture with rhubarb and magnesia was also directed. There was, however, no improvement, and on the second day after the child fell into convulsions. The face was very much distorted, and the convulsions were confined to one side, the other appearing to be palsied. Three leeches were applied to the temples, and the child was put into a warm bath. After a second consultation, four leeches more were applied, and the little patient at length recovered, but was hemiplegic. The paralytic affection, however, disappeared, after the application of a blister to the nape of the neck and vertex. The improvement, though wonderful, was however temporary; the fits of crowing returned on the next day, and in

two or three days, the little sufferer fell into convulsions again, followed by profound coma, from which he never recovered.

The mother had another child in the following year, which fell into convulsions in, I believe, the second month, and died. I did not see it, but mention the circumstence to show the family predisposition to such affections.

Some months after my attendance on the foregoing case. I was sent for by a lady who had lost many children, to prescribe for an infant that had been just seized with convulsions. She had only this little one of seven or eight months old, and another, a weak, emaciated boy aged four or five years, who was suffering with paralysis of the lower limbs from spinal disease out of a family of eight children, most of whom had died of convulsions or hydrocephalus. The child was in a warm bath, and recovered from the fit when I had arrived. It was like the former, of a gross habit, and though still at the breast fed largely. It took bread and milk often, was allowed broths, and sucked most greedily. It certainly did credit to the diet, for it was one of the finest children I ever saw, large, round, plump and rosy faced, with eyes full of light and intelligence, and a disposition full of play. I found it had been attacked in a similar way about three weeks or a month before, and that a scabby eruption, with which the whole head and upper part of the face had been covered was then first observed to decline. It had been teething for some time, and used to dribble a great deal, but this drain of saliva had also lately diminished. The convulsions, as described to me, were general, As the child was very plethoric, three leeches were applied to the head, the gums were lanced, he was freely purged, and small blisters were applied behind his ears. On the following day, although there was no return of the convulsions, he had once or twice, as the attendants described, a slight fit, though without spasms of the limbs. A more minute inquiry convinced me that it was an attack of the crowing disease. The child, they said stiffened, and lost its breath; and although the crowing did not strike them particularly, when I imitated the sound and gasping manner of the infant, they at once recognized the perfect resemblance. I believe, indeed, one reason for the apparent infrequency of this disease is, that it is always spoken of as a convulsive fit by the nurse or attendants, and as the physician is seldom in time to witness the paroxysm, unless he institutes an accurate inquiry,

and obtains a faithful and minute description of the attack, he necessarily confounds it with the common convulsion of infants. I was indeed, for several days in attendance on this little boy, during which he had several returns of the fit, before I had an opportunity of verifying my conclusions, although from the mother's faithful picture of the fit, after her attention was directed to the importance of discrimination, I was perfectly convinced on the subject. The blisters were now kept open with savine ointment; the bowels, which were exceedingly costive and obstinate, were regularly opened; and he was restricted to liquid and farinaceous diet. In less than a week he seemed perfectly well.

On calling to see the child in about a fortnight after, I found him much pulled down; his face pale, his look dull and depressed and he had lost his cheerfulness; there had been, however, no return of the crowing. Somewhat apprehensive of the effects of the debility which had followed the regular purging, and restricted diet, I again allowed the little fellow broth, with some bread or panada; in a few days he began to look up again, and before a week his former bright looks and playfulness had returned. About two months after this, I was once more summoned to see him, and to my great regret found this fine boy had relapsed into a worse condition than he had been in any former attack; the gasping and breathlessness recurred frequently, and were very protracted; and the recovery of the breath, with the loud crowing, took place only after most distressing struggles. He was still teething, his bowels were very confined, and I lamented to see that the eruption on the head and face had nearly disappeared. The same treatment as before was once more resorted to; lancing the gums; the warm bath; repeated doses of active purgatives, and enemata; leeches were also again applied—these last remedies chiefly in consequence of the very plethoric habit of the child. When a sufficient degree of depletion had taken place, the fits still recurring at intervals of two or three hours, or oftener, blisters were applied behind the ears, and injections with assafœtida were administered. On the next day a blister was applied to the neck, and a few drops of laudanum were added to each enema. Other antispasmodics were also administered by the mouth, but without evincing any influence in arresting the frequent attacks of the complaint.—The laudanum was given two or three times in the injections, and did not produce any effect whatsoever, upon

which I did not press it further. The little patient passed on the whole a better night than the previous one, but on the succeeding day suffered much; the attack occurred from the merest trifle, and was attended with appearances of convulsion. Towards noon he appeared stupid and heavy, and the pupils more dilated and sluggish in their movements. As the pulse was quick and feeble, the face pale, and the skin cold, I now gave some wine, and fed the little fellow occasionally with broth. An astonishing improvement followed the use of the wine, such, indeed, as I have not unfrequently witnessed in the advanced stage of hydrocephalus, when the great debility seemed to demand it, but invariably without leading to any permanent good. The insensibility and disposition to fall into a state of coma completely disappeared, the little sufferer looked up again with evident consciousness, and the fits of suspended respiration were shorter and less distressing. In two or three hours after, however, when I repeated my visit, I found all matters worse; the respiration was loud and tracheal, and he seemed again insensible, though still recovering some degree of consciousness whenever the difficulty of breathing occurred. In this utterly hopeless state I was obliged to leave him to visit another patient who was in imminent danger, and learned afterwards that he died towards morning, convulsed.

The only symptom of convulsion which I witnessed throughout was the contraction of the thumbs in the palms of the hands.—The first attack, the one with which the child suffered a fortnight or three weeks before I was called to see him, and that on the day I commenced my attendance, which, however, I was not in time to witness, were both, I believe, instances of common convulsion. I had not read Dr. Ley's papers on this disease until a day or two previous to the boy's death, when I was induced to examine the state of the lymphatic glands particularly. *I found the glandulæ concatenatæ enlarged all down the neck,* but did not obtain permission for a *post mortem* examination.

CASE III.—I met with another case some time after equally interesting. The mother of a fine little boy, aged nine months, during my attendance on its father, almost casually mentioned to me that it had had a strange crowing noise in its breathing once or twice in the night, losing its breath at each time of its occurrence, and blackening up for a few moments in a very strange manner. She mentioned that it had this crowing since

it was four months old, occurring chiefly when it was awakened out of its sleep, or when it became impatient for food and was not instantly gratified. Believing it to be merely the result of passion or quickness of temper, its continuance ever since more or less did not excite any uneasiness in the minds of the parents, until the suspended respiration and darking of the face supervened. The mother was then surprised at my alarm about the nature of the attack and my expression of apprehension that it would terminate in convulsions. The child had had some disturbance of bowels for some days, and was passing greenish or colourless gelatinous stools, perhaps to the amount of four or five in the day, not very liquid nor discharged with irritation. As there appeared to be one or two teeth pushing up the gums were lanced und a grain of calomel with a grain of James's powder was ordered every third hour. It had one or two fits in the course of the day, and in the evening fell into convulsions which subsided on its being got into a warm bath.—As the bowels had been freely moved by the calomel, and the gelatinous stools continued, a warm oatmeal tea injection with a little assafœtida, which however was not retained a moment, and a powder of mercury with chalk and a little Dover's powder was given followed by occasional doses of the milk of assafœtida with some rhubarb and magnesia. At midnight another convulsion fit occurred, which was again relieved by the warm bath. The breath had been suspended in the intervals followed by blackening of the features several times. Although there was neither heat of skin nor thirst, nor flushing of the face or appearance of fulness of the circulation in the head, as a measure of precaution I directed a leech to be applied behind each ear, after which the application of blisters..—A grain and a half of calomel was ordered every second hour, an enema with seven drops of laudanum, and a warm poultice to the abdomen. There was no tenderness there, but I observed that the stools were always preceded by symptoms of pain or uneasiness and forcing, upon which the crowing fit instantly and invariably supervened—the instant rejection of all enemata also argued some degree of irritation, although there was no tenderness upon pressure. It occurred to me that if the warm catasplasms and the opiate enema (should it remain) happened to allay the uneasiness of bowels, the fits might be prevented altogether. In case of failure I left a phial of laudanum with the mo-

ther, and desired her to give from four to seven drops every hour until the gelatinous stools and uneasiness of bowels were allayed. The enema, though repeated two or three times, was, as I apprehended, returned back before the pipe was withdrawn, and as none of the remedies produced the slightest benefit, the fits still recurring frequently, four drops of laudanum were given by the mouth—then five—then seven, after which the child shewed some signs of drowsiness—all irritation and uneasiness of bowels ceased, and this improvement was still more strongly marked by a total cessation of the fits either of crowing or convulsion. On visiting the infant next morning I found it lying quiet, breathing easily and regularly with a less rapid pulse, cool skin and moist tongue—it took its drink well which it could not do in the night, and made its accustomed little cooing playful noise on getting it—the last stool was of a very improved character, and the child's eagerness for feeding shewed evident hunger. It was always rather a full feeder, getting usually three large cups of bread and milk every day, but since the occurrence of the attack I had restricted it to whey and milk and water.—We were now in great hopes that the little patient's life was comparatively safe, at least as regarded the present attack. It passed through the day exceedingly well, was perfectly sensible, and was even animated and almost playful in its looks when fed with whey or milk and slept quietly at intervals—the bowels were moved once or twice with improved appearances, and there was no symptom either of uneasiness of bowels or crowing. When, however, the mother was changing the little fellow's clothes before the fire at night she fancied its eyes looked sunk, and that altogether it was not so well. She fed it however and it took its drink well, but in half an hour or an hour afterwards the extremeties began to grow cold, the whole surface of the body pale and clammy, and the countenance sunk and inanimate; I was instantly sent for, and on arriving found my little patient hopelessly gone—the pulse almost imperceptible and in other respects resembling on infant in the collapse of cholera. There had been no evacuations of any kind to account for this—no vomiting—no diarrhæa—no perspirations. Even the two motions which took place in the whole day were more natural and moderate—not greater in fact than any of those which occurred when five or six occurred in the twenty-four hours. The child was still sensible, taking its drink eagerly and stretching its little

hand for the spoon, though the power of swallowing was evidently becoming more difficult. It expired in about two hours afterwards without pain or struggle.

From the commencement of this case, whenever the crowing fit occurred, it was accompanied by the usual incurvation or contraction of the toes and fingers—there were altogether but two or three convulsion fits. The parents lost a child before by hydrocephalus, and another had slight obliquity of vision, the result of some cerebral affection.

On examination of the body in twenty-four hours after death, we found the sutures completely open, and generally over the whole cranium a deficiency of ossification—so great indeed, that the frontal, parietal, and occipital bones were readily divided with the scalpel—the veins ramifying on the surface of the brain appeared to be turgid, but it was throughout its whole extent perfectly healthy in its structure as far as we could ascertain.—The only appearance that we could consider morbid was the proportion of water in the ventricles which amounted to about six drachms. The abdominal viscera were also examined, without our being able to detect either in the liver or large or small intestines any indubitable marks of disease—the latter were for the most part pale and somewhat distended with air.

A fourth case happened in the practice of my brother, who has given me the following account of it. "In the year 1835 M, W., a child a year old, began to be affected with derangement of bowels, white tongue, loss of appetite, and diarrhœa, attended with thin, gruel-like discharges of a pale colour; these attacks, which were at first attributed to difficult dentition, were usually got over in a week or ten days, but their repeated occurrence produced a degree of delicacy of look, and softness of fibre, and very much checked her growth. In the autumn of that year, being at the sea side, she got so severe an attack of diarrhœa, that for a day or two her life was despaired of; she came home in a very delicate state, with pallid look, white tongue, pale-coloured alvine discharges, which were now, however, of a healthy consistence, and she had lost her relish for all food except milk and thin gruel. In this state she was attacked with nervous startings, and also occasionally with what the mother called 'croupy breathing,' (laryngismus stridulus of Dr. Ley;) this last symptom was never so violent as to suspend

the breathing entirely, or cause much distress. It was in other respects the loud sonorous inspiration so well described by Dr. Ley, always sudden in its onset, and passing off entirely in a minute or two. This state of things lasted some time, and during the continuance of it she was attacked with convulsions, which came on frequently during a day and a night, and which, after she had been blistered and purged, were at length subdued by an enema with fifteen drops of laudanum repeated once or twice. As the nervous startings, the 'croupy breathing,' and the convulsions, were all considered connected with that irritable and over-sensitive state of the nerves which is often conjoined with debility, the occasional use of a sedative with tonics was recommended, and (a tonic mixture, with some tinct. opii, having been found to disagree, by interfering with the effect of the necessary opening medicine) the sedative was usually administered in the form of an enema, with fifteen drops of laudanum, whenever the startings or "croupy breathing' shewed themselves. Under this plan, and by attention to the bowels, she passed six months without any return of the convulsions, and was rarely affected with the starting and croupy breathing, which were always watched and when they did occur subdued by the enema with tincture of opium. She had a very capricious appetite all this time, the tongue usually coated or white, and for a considerable period she would take nothing but bread soaked in wine, on which regimen she gradually, however, gained some flesh and strength. Calling at the house one morning, I found her bowels had been rather confined for some days before, and she had a fit of the croupy breathing in my presence, though not a very violent one. When it was over, and the child was quiet again, the mother was expressing her apprehensions that this was premonitory of a convulsive attack, and asked me if I thought so. I gave an answer tending to quiet her fears, but had scarcely done speaking when the child had a violent convulsive fit, which came on without any of the croupy breathing, and lasted nearly a minute. I put a blister on the nape of the neck, ordered some opening medicine immediately, and remained in the house three hours, during which time she had no return either of croupy breathing or convulsion. I was obliged to go eighteen or twenty miles off that day, and on my return on the next, at noon, found the family in the utmost distress, the convulsions had returned re-

peatedly during the night, though the bowels had been well moved, and the blister had risen. The fits were growing more violent and very frequent, returning every half hour or twenty minutes. I applied three leeches to the temples, gave the laudanum enema, and applied the stimulating dressing to the blisters. The enema was repeated at intervals of about an hour, being usually given whenever a fit occurred of more than ordinary violence. After it was given five or six times it had an evident effect on the paroxysms, which though not lessened in frequency were very much diminished in violence, and indeed were now very slight. The child at this time had a very exhausted appearance, her face was pale, her pulse rapid, and there was so much insensibility that she could scarcely be got to swallow the drink which was occasionally put between her lips with a spoon. The insensibility was perhaps due to the opiate enemata, but as the convulsive paroxysms still returned, though not so violently, and the comatose state was one to which the case naturally tended, though no opiate had been given, I feared if it was allowed to continue, and that my inference proved correct, the time lost would be irretrieveable. The child was already blistered, and as new blisters would be too slow of acting, it occurred to me as an alternative to make an opening along the sagital suture, down to the bone, and place a dossil of lint in it, wet with spirits of turpentine. She showed some sensibility to this by movements of the head from side to side; and though there was some tendency to the fits for two or three hours afterwards, they were so slight as scarcely to attract notice. The insensibility wore gradually off, and by the next evening she was able to take some milk and gruel, and recovered from all the effects of the attack, except the debility.

"The child lived eight months after this attack, but in an extremely delicate condition; her growth was checked; she showed a disposition to rickets in the limbs; acquired a double lateral curvature of the spine, and though nearly three years of age could neither speak nor walk. During this interval she had occasionally, but very rarely, returns of the croupy breathing in a slight degree, and sometimes returns of the nervous startings, which symptoms were always attended to and subdued in the usual manner when they did occur. I could never perceive in

this child any appearance of swelled glands in any part of the neck during her illness, nor any where else except beneath the occiput, where some small ones could sometimes be felt, produced probably by the discharge from the head, which was kept up to the time of her death. This event occurred after an attack of diarrhœa and vomiting, much slighter in degree than many that she had had previously. There was no examination of the body."

As the intimate pathology of these cases is likely I fear for a length of time to remain a mystery, it becomes a matter of extreme importance to ascertain in how far the pathology of the nerves in general and the inference deduced from analogy may determine the general question whether they belong to the class of paralytic affections, and are dependent on the pressure of the recurrent nerves by lymphatic glands; or to convulsive disorders, and arise from some change occurring at the origin of the par vagum, chiefly affecting the superior laryngeal branches. The late Dr. Hugh Ley has already in a very elaborate work considered the subject in all its relations. He has indeed brought such a mass of information together in support of his views, and reasoned so ingeniously, that he has I believe made converts of a great many of the profession to his opinions. There are, after all, difficulties which Dr. Ley has by no means satisfactorily got over, and which are sufficient to throw doubts on his whole hypothesis.

The physiology of the parts engaged in the disease, and many facts illustrative of the general pathology of the nerves, give an extraordinary speciousness to Dr. Ley's views, which we shall not find so fully supported on a more minute examination. I shall, however, first consider what value should be attached to one or two symptoms, the presence of which, in the majority of cases, seems undeniable, and with respect to one at least is, in Dr. Ley's opinion, necessary as the exciting cause of the malady; I mean eruptions on the scalp, ears, or face, and enlargement of the lymphatic glands in the neck or thorax. When we reflect how rarely the most alarming symptoms of the disorder, the suspension of respiration and crowing, are found in connexion with disease of the head, which is so common; how impossible it is to account for that unfrequency on any supposition of the affection originat-

ing in disease either of the superior laryngeal nerve, or of the origin of the par vagum; and how perfectly it seems to be explained on the opposite view, as arising from pressure on the recurrent branches by enlarged lymphatic glands—when further we find that most cases are attended by eruptions about the scalp, ears, or face, with enlarged glands just in the course of the recurrents; when we know that injurious pressure may possibly occur, the effect of which must be, diminished power in the nerve; and lastly when we are aware that the consequences of such defective power or paralysis must be difficult if not impossible respiration, we must admit that a very specious case is made out in favour of the only hypothesis which offers a solution of the difficulties involved in the pathology of laryngismus stridulus. It is, however, worth while to examine the seperate links of this imposing chain of reasoning.

It must strike every observer with regard to the enlarged glands, that as they are very common in children, as common almost as the disposition to struma, the crowing disease should be common also. Dr- Ley anticipates this objection, but he puts it only as applying to the immunity of adults from such an affection, which he accounts for by saying: 1st, adults are comparatively free from the causes which produce such enlargements; 2ndly, where the trachea has acquired the adult size, it gives more protection to the recurrent; and 3rdly, the larynx and glottis of the adult are much larger than those of the infant. These arguments, strong as they are in explaining why such an affection should rarely occur in an adult, by no means satisfy us as to his perfect immunity: amidst all the tumours to which adults are subject about the neck, it is rational, if Dr. Ley's hypothesis be true, to demand the citation of at least one fair case of laryngismus stridulus arising like it, resembling it, and terminating as it does in the infant. The real difficulty, however is not about adults but about grown children, between whom and mere infants there can exist no extraordinary disproportion in the size of the trachea, larynx, or glottis; and in whom the disposition to enlargement of the lymphatic glands is, I should say, rather greater. Why are not grown children attacked with laryngismus stridulus? I have never seen the complaint except in mere infants; all Mr. Robertson's and most of Dr. Ley's cases

appear to have been under two years of age; and Dr. Underwood speaks of it under the head of inward fits, as one of the disorders of early infancy.

The mere presence of enlarged glands in this complaint, may certainly, as Mr. Robertson of Manchester suggests, be a coincidence only. Dr. Marsh asserts that he has never seen the crowing disease in any but infants of strumous habit, and with such, enlarged glands are usual, whether they suffer from any such affection or not. Strumous ophthalmy is very frequently attended by enlarged glands, and disappears as the enlargement subsides, yet it furnishes no argument of the dependance of the one on the other, both being the result of a common taint in the system. Enlarged glands are indeed, apt to appear with most strumous diseases of infants. Mr. Swan states that he has frequently found the glands within the chest enlarged in children who died of hydrocephalus, yet I question whether there was any such phenomenon as crowing in a single one of these cases.

Mr. Robertson very fairly inquires, "is it probable, that in a soft yielding structure like the throat, absorbent glands should, by pressure on the par vagum and recurrent nerves, cause a diminution or extinction of nervous energy?" If the pressure could be supposed sufficient, no one could doubt the effect; the question for consideration is, can it be so? The only satisfactory instances of such effects from such pressure adduced by Dr. Ley, are those cited from Sir Charles Bell's work, in which partial paralysis of the face occurred from the pressure of an enlarged gland on the seventh nerve between the mastoid process and angle of the jaw. Even in these, however, the analogy is not perfect, as one may readily conceive that an equal degree of pressure would occasion far more important effects in that situation, than in the lower and less resisting parts of the throat. If the pressure of enlarged glands could in the latter situations so readily affect the energy of the nerves, it is wholly inconceivable that it should not more frequently happen in strumous children with enlarged glands; and admitting for a moment, the assumption that the effects of the pressure would be immediate, it seems extraordinary that it should not often take place in those violent strainings or twistings of the neck, which in their games and exercises so continually occur. This last observation, I have

made, because Dr. Ley states, in explaining the occasional absence of the crowing, "that glands not constantly in contact with a nerve, may, during the varied contractions of the muscles of the neck, in crying, coughing, laughing, or sudden twisting, produce at once contact and compression *when temporary asphyxia will be the result.*" I do not, however, believe, that momentary pressure on a nerve, however great, occasions any inconvenience at all, of which we have every day evidence in our own persons. To impair the energy of a nerve, the pressure must be continued for some time, and when once impaired, it is a considerable time before it is again recovered, although the pressure be removed.

This brings us to the consideration of the paroxysmal nature of the disease, which Dr. Ley acknowledges he has always looked upon as the strongest argument for its spasmodic character.—Adopting M. Swan's explanation of the paroxysmal nature of all convulsive and painful disorders of nerves, quoted by Dr. Ley, "a nerve cannot at first bear a diseased action without rest, any more than a healthy one, and therefore the diseased action after a certain period, ceases to make any impression; but after this rest, the nerve acquires fresh powers, and is again fitted for the same action. In palsy, on the other hand, the pressure being permanent, the loss of sense or motion, or of both, is permanent also." If this were universally true, it would be altogether fatal to Dr. Ley's supposition of the paralytic nature of laryngismus stridulus, as he would find it impossible to reconcile the rapid recovery, easy respiration, and long intervals of perfect relief between fits occurring on the same day, with permanent defect of power. But it is only true, he says, as far as the nerves of sensation and volition are concerned; for in paralytic ailments of the muscles supplied by respiratory nerves, the palsy, though continuous, is not constantly manifest; and in proof of this, he instances the effect of pressure on the portio dura, the defective power occasioned by which is not observed, till in speaking or in emotion, or difficulty of respiration, vigorous action of the muscles is required. So in like manner he says, in the glottis "the effects of the enfeebled, if not paralyzed, state of its opening muscles, are only observable in those more vigorous efforts which are made when the respiration is hurried or impeded, as

in fright, fits of anger, sudden awaking from sleep, in consequence of some external impression, and in screaming, crying, coughing, &c."

It appears to me, that the whole question of the spasmodic or paralytic nature of the disease turns chiefly upon the correctness of this distinction which Dr. Ley has drawn between the voluntary and respiratory nerves. To take the instance which he has himself put forward in illustration, that of the seventh nerve, he has certainly fallen into error, probably in consequence of having seen no case in which it was perfectly palsied. In perfect or even in very considerable palsy of the portio dura, I have always seen the face permanently dragged to one side, whether the muscles were in active contraction or not. In slighter cases, where the energy of the nerve is not much impaired, I am aware the defective power is not observable until the opposing muscles are thrown into vigorous action, but this would be equally true, if the opposing muscles were supplied by purely voluntary nerves. The law that if the pressure is permanent, the defect of power must be permanent exactly in the same degree, is in fact true of the portio dura, as it is of all other nerves; and referring this law to the glottis in paralysis of the recurrents from pressure whatever effect is produced, and in whatever degree, must be as permanent as the pressure that occasioned it. I do not here take into account the greater defect of power observable in the palsied muscles in one side of the face, when the opposing ones are in vigorous action, or those of the eyelid when we attempt to close it, or in the glottis when the closing muscles are spasmodically or energetically contracted. In these instances the effect arises from active contraction of the healthy muscles, not solely from the palsy of the others, and therefore can last only as long as muscular action usually lasts—a few moments. But even this effect, in its constant recurrence, if not in its continuousness, bears a permanent relation to the paralytic affection, since no action of the unaffected muscles can take place without its appearing. It is clear, therefore, that if we are to explain the paroxysmal nature of the crowing disease, by any such effects of loss of power in the recurrent nerves, we must assume that when once the paralysis is more or less imperfectly established, whatever ill result or difficulty of breathing is at any time observable in

laughing, crying, screaming, fright, &c., should invariably return with every recurrence of these, and a certain amount of mischief, however slight, should always be distinguishable when at perfect rest; just as the slighest defect of power in the portio dura is observable in the slightly parted eyelid while the patient sleeps. But the fact seems entirely otherwise; children affected with this disease pass days without the slightest appearance of illness, between two fits of suspended or difficult respiration, during which they laugh and cry, and are excited as usual.

The foregoing observations must sufficiently display the error Dr. Ley has fallen into with regard to the effects of palsy of the recurrent nerves in occasioning the phenomena of laryngismus stridulus. On the whole, after all the consideration I have devoted to the complaint, and having, I think, given other ingenious arguments of Dr. Ley their full weight, I must still confess myself a disciple of the older doctrine, that the affection is one of spasm or partial convulsion like cramp, rather than of paralysis. The fact of its being frequently benefited by antispasmodics, with which Dr. Underwood tells us he latterly cured most cases, and by anodynes, as opium, hemlock, cicuta, &c,, recommended by all modern writers on the disease, favours this view; the circumstance of the sudden occurrence of the gasping and crowing on washing with cold water, laughing, crying, or agitation of mind, also supports it, as well as the almost universal coexistence of the carpo-pedal contractions, and the frequent termination of the complaint in convulsions. But above all these, as a strong analogical evidence for its spasmodic character, I place its paroxysmal nature, and the manner in which the paroxysms occur. The office of the superior laryngeal nerves would lead us to expect a disposition to spasmodic action on the least irritation or excitement, recurring at irregular intervals, dependent of course on the return of the irritation or excitement, but far more on the increase or decrease of the susceptibility of the parts, and disposition to spasmodic action. Dr. Mayo very justly remarks, that " a frequent disorder in parts endowed with acute sensibility, like the mucous surface of the larynx or eyelids, is an increased susceptibility of the sentinent surface, and a tendency to spasmodic action in the adjacent muscles, which usually

act from impressions received upon it. Thus in the urethra a morbidly sensible state of a part of the mucous membrane produces spasmodic stricture, or a continued contraction of the surrounding fibres of the accelerator urine. In like manner is produced spasm of the glottis, in cases of ulcer within the larynx and in hydrophobia." The muscles supplied by the superior laryngeal nerves are the sentinels of the chink of the glottis, as the orbicularis is of the chink or opening of the eyelids, and their action is directed by an exquisite sensibility of the parts, which is at once manifested on the attempted entrance of anything injurious to the lungs, whether it be a drop of water, or a volume of mephitic gas. One can well understand how dangerous any morbid increase of the sensibility of such nerves at their extremeties, or any existence of irritation at their orgin, might prove; why the danger should occur in irregular paroxysms; and why the exciting cause which occasioned them on one day should be altogether powerless on the next. If it be enquired further, why such a dangerous result as the suspension of respiration in the crowing disease does not then occur more frequently, it can only be replied, that we are wholly ignorant of the morbid condition which disorders the functions of those nerves, or whether it exists at their extremities, or their origin in the medulla oblongata. If we suppose the affection to be organic, we should find it more difficult to account for the occasional recoveries under very mild treatment, than the usual fatality under the most active. If it be functional, and therefore symptomatic, we can better understand why it might depend on a variety of causes; at one time upon an affection of the head, at another of the bowels, at another upon dentition; we can comprehend, too, how these several affections, influencing peculiar pre-dispositions, may in one child occasion hydrocephalus, in another convulsions, in a fourth, that more rare infantile disorder, the crowing disease.

This seems to be very much the view taken of the affection by Dr. Marsh, and more lately by Dr. Stokes. The disease as the latter physician observes may shew itself as a simple spasmodic affection of the larynx, *independent of any other perceptible lesion;* but this is the rarest case. In others, it is connected *with the irritation of dention or deranged digestive functions;* while in a third class, it is symptomatic of primary cerebral

disease. In this last the spasm of the glottis is as symptomatic of the cerebral disease as the convulsions of the extremities.

In the treatment of this complaint, the obvious preliminary to a judicious plan of cure is to ascertain, by a careful examination of all the functions of the body, whether there be anything wrong to which we could trace its origin. When any such causes or complications, whether in the head chest or abdomen, are found to exist, their removal should form our first object, but always with reference to the strength or tone of the system, which in an affection invariably found in connexion with a strumous habit, cannot be lowered much without injury. The maintenance of the general health and strength seems to be on all occasions a matter of importance, but still more so in those cases in which no complication exists, and where probably the complaint very much depends on debility, in connexion with either a cachetic state of the body and wasting; or preternatural irritability with plethora. From my experience of the complaint I am disposed to believe that no treatment, entirely founded on the supposition of its being a pure cerebral affection, can be successful, and I fully agree with those practitioners whose great anxiety is to restore the natural tone and firmness of the system, giving tonics where there is deficiency of power, and antispasmodics and anodynes where there is great irritability. To attain our object, however, it must always be held in mind that in the chronic affections of children, the exhibition of medicines is secondary to the management of their diet and change of air. The former should be regulated with the utmost care, and in many cases Dr. Marsh's suggestion with respect to a succession of good nurses during the whole period of dentition might be attended with advantage. Change of air not only in this complaint but in all obscure chronic cases is invaluable, being in fact the only remedial measure about the beneficial influence of which there can be no question, however our views of the nature or pathology of the disease may alter from time to time. I have often thought that in such complaints, and still more in those which are supposed to be endemic, it would be most desirable, in every instance where it was at all practicable, to treat the patient in an atmosphere differing as much as possible from that in which his disease originated. I believe many cases of croup and cholera, such at least as are not of the rapid type, might be treated successfully if removed into a new

atmosphere, which are utterly hopeless when the cure is attempted in the locality, where the patients sickened. Whether a child affected with laryngismus have change of air or not, however, it is essential that he shall sleep in a cool and airy apartment, and that when the weather permits he shall be as much as possible in the open air. There is no one point in which children are more mismanaged than in the arrangement of their sleeping apartments, which are commonly the most close and confined in the whole house.

Dr. Merriman has recommended the use of continued purgatives, so as to procure at least two free motions daily. When the child is plethoric, or is otherwise likely to bear evacuations well, it would, I have no doubt, be an advisable plan; indeed in young children the bowels are usually moved as often as this in the twenty-four hours with advantage, but the recommendation, in any case, should be followed with constant regard to the strength of the little patient, and the apparent effect. He has also suggested the use of soda or burned sponge, probably with the same view which led Dr. Ley to propose the preparations of iodine, to diminish the size of the enlarged glands in the neck and chest, to the pressure of some of which on the recurrent nerves, he attributed the disease. Doubtful as I am of the correctness of this opinion, I should think it judicious to administer the hydriodate of Potass in minute or tonic doses, especially as it could in no sense interfere with other remedial measures on which I should perhaps set more value. It seems to be a fair indication to endeavour as much as possible to counteract or overcome the general strumous disposition in those affections, which are seldom or never found to occur except in conjunction with it. When we have satisfied ourselves that there is no evidence of cerebral congestion, or of abdominal disorder, and that there exists no irritation from dentition, we might, I conceive, place much dependence on the influence of carbonate of iron, or of oxyd of zinc, in preventing recurrences of the attack. Any of those remedies in fact, which evince so much power in preventing the returns of many descriptions of epilepsy, or in relieving spasmodic affections of particular muscles might be tried. The subjects of this alarming disorder are usually too young to bear the free or effective administration of opium, camphor, or other narcotics, but if I were to place much reliance on any medicines of that class, I should be most disposed

to make trial of the tincture or extract of Indian hemp, which through the industrious and interesting researches of Dr. O'Shaughnessy of Calcutta, have been lately added to the list of our materia medica. This valuable medicine, from this gentleman's account displays extraordinary power in controuling spasmodic action without occasioning the congestion of brain, or constipation produced by opium, and may be exhibited freely to the most delicate infant without danger.

I have offered these few unsatisfactory observations without wishing to attach more importance to them than they merit. I believe both the pathology and treatment of the disease are still very uncertain, and that it will require all the consideration and inquiry which observant practitioners can bestow upon the subject for many future years to attain a just knowledge of either. The following summary of the amount of our present information and of the facts connected with the disease, may be useful to subsequent inquirers.

By the concurrent testimony of almost all who have noticed the affection, it occurs for the most part, if not wholly, in strumous habits.

It is frequently found in connexion with enlarged glands in the neck, and perhaps in the thorax.

It is frequently found in connexion with eruptions on the face, ears, or scalp.

It frequently terminates in convulsions, and is sometimes, though very rarely, ushered in by them.

It is met with in families in which children are subject to head affections or convulsions, but who have also the strumous disposition.

It is sometimes met with in connexion with an apoplectic or comatose state from the commencement, as in the cases of crowing apoplexy which I have described.

In a great proportion of the cases which terminated fatally there was no symptom of head affection through their whole course, beyond the occasional fits of breathlessness and crowing: and the children were as well apparently, a few moments before death, as they were previous to the first attack of the disease, or as any children could be.

The complaint is sometimes, but rarely, attended by cough or permanent difficulty of respiration.

I believe it may be said that from one third to half of all the cases of which we have any account, terminated in death. But this great mortality may perhaps in some degree be attributable to the over active treatment pursued in many instances, from mistaken notions of the congestive or cerebral nature of the disease.

PROBLEM IX.

BY W. GRIFFIN, M. D.

DOES SUFFERING NECESSARILY IMPLY SELF-CONSCIOUSNESS? ARE SENTIENT BEINGS NECESSARILY PERCIPIENT?

SECTION I.

It is a fact familiar to almost every medical practitioner, that a person in an apoplectic fit, if pinched severely in the hand or foot, will sometimes retract the injured limb, and perhaps utter a low groan or expression of suffering. We have the strongest grounds for believing that there is a total unconsciousness in this state, not only in the obvious insensibility of the special senses and the utter impossibility of arousing them, but in the invariable want of all recollection on the part of the sufferer, after recovery, of having experienced any pain or sensation whatsoever. It often occurred to me that it was difficult, if not impossible, to reconcile facts of this nature with the received physiological or metaphysical doctrines on the subject of sensation and consciousness; and to this conclusion I was eventually disposed still more to accede some years since, on reading the accounts of the experiments of Le Gallois, Fleurens, Magendie, and others, on living animals. It had been long universally believed that sensation, consciousness, and volition, were mental acts or functions connected with the cerebral lobes or true brain; but those physiologists have shewn, that although self-consciousness, and of course memory and the association of thought, are lost on the removal of those lobes, and as a consequence, the capability of originating motion, an animal so mutilated may yet live long, is capable of sensation and of the instinctive actions most directly linked with sensations,

shews a power of effecting regular and combined movements on external stimulation, and if pushed forwards will continue to advance, even after the impelling power has been wholly expended. It swallows what has been put into the mouth, moves its legs when irritated, and its wings when thrown into the air; but when not excited by any impression made on the senses, appears in a state of stupor, gives no signs of recollection, even of sensations just felt, nor of such emotions as sensations were wont to excite, and cannot seek its food, nor even avoid obstacles thrown in its way.

It has been further proved that consciousness in its fullest sense, perception, memory, and the associations of thought—the faculties lost in these experiments—belong strictly to the cerebral lobes; since, if these are left untouched, and the cerebellum removed, they remain uninjured. An animal deprived of its cerebellum, loses all power of regular and combined movements ; if placed on its back, it tries in vain to turn round; it perceives and is apprehensive of blows with which it is menaced, hears sounds, seems aware of danger, and makes attempts to escape, though ineffectually: in short, while it retains uninjured sensation and volition, it looses all power of rendering its muscles obedient to the will.

It appears very extraordinary, in reflecting on these experiments, that after the removal of the brain or cerebellum, or both, sensation and apparent volition should remain; for if it be true that consciousness, perception, and memory, are confined solely to the cerebral lobes, and of course extinguished in their removal, we have either the option of supposing that the motions we attribute to sensation and volition are altogether erroneously so attributed, or that there are in the system two independent centres of consciousness (a supposition which I believe all metaphysicians would look upon as an absurdity); or, finally, that the motions indicate neither sensation nor volition, but are entirely automatic.

M. Magendie has shewn, that after the brain and cerebellum are removed from the adult hedgehog, leaving the medulla oblongata entire to above the apparent origin of the fifth pair of nerves, the animal cries if a hair of its whisker be plucked, or if vinegar be held to its nose, and strives with its fore-feet to rid itself of the object which incommodes it.

Mr. Grainger had the brain removed in a young puppy, which

was then put to a large bitch, not to the mother, but which was suckling at the time. The puppy, on touching the mamma, threw up its nose and moved the mouth, trying to get hold of the nipple, which, however, was too large, Mr. Barron, who was performing the experiment for Mr. Grainger, then moistened his finger with sugar and water, and put it into the mouth; when the puppy sucked, the tongue being wrapped round the finger. The same experiment was performed, with a similar result, on another puppy; but the most remarkable fact in the last case was, that as the puppy lay on its side sucking the finger, it pushed out its feet in the same manner as young pigs exert theirs against the sow's dugs.

Lest it should be imagined that the brain was not perfectly removed in these experiments, it is sufficient to mention that motions quite as extraordinary, and apparently as much the result of volition, occur in animals after the head has been entirely removed. A frog left at rest after decapitation, will draw up its legs and assume the sitting posture. A decapitated serpent or salamander, when the surface of its body is touched, will turn its headless trunk in the direction of the irritation; and a fowl similarly mutilated, will run or fly in a staggering or fluttering manner across the floor, and, after the first convulsive struggles subside, will, if touched with a sharp instrument or a hot iron, wring the injured leg or wing with all the usual manner of pain.

Some of the most celebrated physiologists who were engaged in such experiments, impressed with an irresistable conviction that such phenomena were the result of sensation, were constrained to acknowledge the possible existence of two centres of sensation and voluntary motion, however inexplicable. Others virtually admitted as much, though in a qualified and obscure way. A third and larger number resisted the evidence of expression of pain derived from the resulting muscular motions, even when such motions were obviously adapted to a purpose. They denied the existence of either sensation or volition, as connected with them, and were satisfied to view such remarkable phenomena as instinctive or automatic, analogous to those movements which are excited in internal organs or vessels by the stimulus of their fluids. More lately they have been considered as altogether the result of impressions on the extremities of the sentient nerves, propagated

to the spinal cord and reflected back through the motor nerves. But in no instance has the correctness of the notions usually attached to the terms consciousness, sensation, perception, or volition, been questioned.

In support of the conclusion that the movements in all these cases are automatic or reflex, it is argued, that as the head after its removal from the body, also retains its excitability or apparent sensibility, evinced by the motions of the ears when pricked or touched with a hot wire, we could not admit of sensation and volition in the trunk without acknowledging it likewise in the former; which would be, in fact, acknowledging the metaphysical absurdity already referred to, of two centres of consciousness coexisting in a single being. It is also said, on the same grounds, that as we have the strongest evidence for believing that consciousness, and therefore volition, are functions wholly confined to the anterior hemisphere of the brain, any phenomena, however remarkable, which survive the destruction or removal of those parts must depend upon some other influences; that they are not more extraordinary than the results of other instincts which are neither connected with nor accompanied by sensation; and there is not a single instance af automatic motion in parts supplied by spinal nerves, which may not be accounted for on the demonstrated property of the central axis to transmit impressions from the excitable to the exciting nerves, at any part where they are connected with it, independently of the rest.

It must appear, however, on reflection, that even admitting the facts upon which the foregoing inferences are founded—admitting the absurdity of supposing a second centre of consciousness in a single animal, and admitting that those supposed instinctive or automatic motions are the results of a reflex property in the cord, by which, when impressions are received by excitable nerves, actions are originated by motor ones without reference to the brain, the essential organ of consciousness; the assertion must still be regarded as pure assumption, that sensation is not in any degree concerned in movements so strongly characteristic of pain, so long as no more positive evidence of it is adduced than the mere absence of individual or self-consciousness. Magendie, Fleurens, and Rolando, have proved to us, that though memory and perception, and of course consciousness in its fullest sense, are lost in the destruction of the cerebral lobes, all the indications

of common sensation and the power of effecting combined and regular movements on external stimulation, remain. Is it not, therefore, the natural and obvious inference that common sensation, which has survived the destruction of the cerebral organ, can have no dependence whatsoever on it?—and are we to reject this inference because metaphysicians, before they had sufficient data to reason upon, proposed speculative definitions of it, always confounding it with consciousness, and virtually identifying it with perception?

If we were to reject the evidence of analogy in favour of the existence of sensation in such experiments as have been detailed, it would be exceedingly difficult to prove that any living being except man suffers. In man we have not only the evidence of close analogy, but of universal testimony to convince us. In the brute creation our inferences, absolute as they may be, are deduced wholly from the resemblances of their cries, or writhings, or other indications of pain, to our own. Yet these are so constantly observed throughout the animal creation, in connexion with such injuries as would occasion them in ourselves, that our conviction respecting their sufferings are in no degree less certain than when the same signs are observable in human beings. It is, indeed, no slight proof of the truth and reasonableness of this ready assent to strong and constant analogies, that the same ready belief is intuitively accorded, and pain inferred, from cries and complaints in all living creatures by the infant, long before there exists any possibility of its forming deductions from analogy on such subjects. The same instinctive belief operates powerfully even in animals themselves, who evince the greatest degree of uneasiness when they hear the cries or complaints of their offspring or of others of their kind.

There are even few who have witnessed experiments on recently decapitated animals, able to resist the strong impression of the existence of sensibility created by their movements on the application of stimuli. When these movements are said to be instinctive, or automatic, or reflex, and dependent solely upon impressions rather than sensations, it is merely assuming the point at issue, since the existence of instinctive or reflex motions offers no proof that they are independent of sensation, and, in fact, were admitted and believed to be dependent on it, by the most ce-

lebrated physiologists, before the doctrine of reflex movements by impressions assumed its present systematic form.

It is rendered sufficiently probable by the experiments of Fleurens, Bouillaud, and Magendie, that thought and sensation, or the intellectual and sentient functions reside not only in different but in distant parts of the nervous system. It seems to be admitted on all hands as nearly certain, that consciousness, memory, perception, and the association of thoughts, are confined to the cerebral hemispheres; while Professor Alison states, "it is now satisfactorily ascertained that no part of the brain higher than the corpora quadrigemina, nor of the cerebellum, *is essentially concerned in sensation.*" Consciousness, therefore, or at least the faculties which are essential to it, belong to the cerebral lobes; sensation, or all the physical conditions necessary that impressions may be felt, and that voluntary efforts may excite muscular contraction, belong to the spinal cord. When, with these admissions, we have the facts before us that the former class of functions, the purely mental, with the organs to which they belong, may be utterly destroyed, and yet the latter or sentient functions remain uninjured, it must be acknowledged that we have strong evidence for doubting the correctness of the present metaphysical meaning attached to the terms consciousness and sensation.

Setting aside for a while any considerations on the subject of consciousness, let us inquire strictly into the value of the arguments upon which we conclude that sensation subsists after decapitation, and the degree in which it is possible to account for the phenomena indicative of it on any other hypothesis.

Independent of the instinctive belief already alluded to, which is probably mixed up with all our inferences in favour of the existence of sensation, wherever we perceive the usual indications of pain, we are influenced, partly by the correspondence of the movements which take place on injury after decapitation with those induced by similar injury in the perfect animal—partly by their adaptation to a determinate end—and, above all, by their occurring spontaneously, and, as it would seem, originating in pure volition.

The correspondence of the movements in headless or brainless animals when injured, with the movements of unmutilated ones, is so remarkable that it would be unnecessary to offer an observa-

tion on it, if it had not been much questioned, and, in fact, denied by some late physiologists. Mr. Grainger mentions a salamander in which the posterior extremities were paralyzed by division of the spinal cord, so that "by proceeding cautiously and not allowing the animal to see the approach of the hand, an entire leg was cut off with a pair of scissors without the creature moving, or giving any other expression of suffering." But in this case it is as difficult to understand why there was no reflex movement as why there was no pain, even taking the loss of the limb into account, in which of course the most violent results of the injury would have been shewn. Perhaps much importance cannot generally be attached to the absence of sentient or reflex phenomena in those experiments, where there has been much mutilation, as it may be dependent on loss of sensibility or of excitability from exhaustion. Volkmann was so sensible of this, that he deduced his proofs from cases only in which motion occurred, not from those in which it failed, it being evident that reflex motions could take place only where the organic conditions were fulfilled; whereas these must often fail on account of the great destruction by the knife. The instances would be more to the purpose in which the same stimulus applied before decapitation and after it, did not occasion precisely the same results, if any such could be well attested. In a frog, Mr. Grainger states that the application of heat to one of the extremities, either anterior or posterior, excited a simultaneous motion in both, an effect he asserts quite different from that produced in a perfect animal *in which the injured limb only is retracted.* As Mr. Grainger, however, in detailing other similar experiments, says merely "*in most instances,*" it was found that only the leg belonging to the foot which was irritated was thrown backwards, and as I have myself seen, in the perfect animal as well as in the decapitated, sometimes the fretted leg and sometimes both retracted when only one was pricked with a sharp instrument, attributable perhaps to the varying intensity of the stimulus, his objection loses much of its validity. In an experiment performed on a rabbit by Mr. Mayo, it is said, that when the foot was irritated, the movement of the limb was exactly similar to that which the animal would make if in indisputable possession of its sensation. In order to ascertain the correctness of this statement, Mr. Grainger mentions that "he pricked the hind foot in a rabbit, the cord of which was entire, when the animal

moved the limb to avoid the irritation; but upon dividing the cord, and pricking the under part of the foot, a most violent motion was excited, and both legs were thrown back. Those gentlemen who were present, he states, were particularly struck with the difference of the movements in this rabbit before and after the division of the spinal cord."

In comparing the results of injuries inflicted on parts which are still in connexion with the brain with those inflicted on parts in which it has been cut off by division of the cord, Mr. Grainger seems to forget that in the one case he has the movements of pain, controlled or modified by volition; in the other, the simple instinctive results of pain without any such complication, and perhaps for that very reason the more energetic. It must not be inferred that arguments tending to prove the existence of sensation in parts separated from the brain go also to establish volition, which would necessarily imply the presence of mental as well as sentient functions; and with this understanding, a *strict* comparison could only be instituted, or *precisely* similar results expected, in dividing an animal which in its perfect state has no brain, and stimulating the separated segments. When animals possessing brain are decapitated, we can expect to witness only the instinctive effects of pain on stimulation, those movements most closely linked with their sensations, and these can bear no nearer comparison with the results of like injuries in perfect animals of the same kind, than as they are both admitted to be characteristic indications of suffering, and such as are known to be instinctive with the animal. Mr. Grainger seems to think the motions resulting from injury are more violent after decapitation than before. I have convinced myself of this in experimenting upon two kittens which were taken out of a pond in a very enfeebled state, where they had been flung for the purpose of drowning them. Pricking the extremities excited only the feeblest motions previous to decapitation, but afterwards the slightest touch of a sharp instrument occasioned ready retraction of one or both limbs. Dr. Volkmann states that a stimulus produces reflex motions in decapitated amphibia, which, previous to decapitation, had no such effect. That these differences are partly attributable to the influence or operation of the will, as already suggested, there can, I believe, be little doubt. There are few persons who have not experienced sudden involuntary starting of the muscles of the limbs or of the whole

body, and such motions are always found to take place most readily when the cerebral system is in a state of exhaustion or rest, as after great fatigue, or when dropping off to sleep, just at the time when consciousness and volition are disappearing. A gentleman affected with partial paralysis of one side once mentioned to me, that whenever he put out the weak limb in progression it always took too long and too forcible a step—going off, in fact, with a jerk, for want of the due control of the will, Such facts sufficiently prove that the influence of the voluntary powers of the brain is constantly acting, and perhaps more frequently in controlling or modifying instinctive or sentient movements than in supporting or increasing their energy. I do not believe, however, that the absence or presence of the cerebral influence or operation of the will can explain altogether in all instances the differences observable in the effects of stimuli applied to perfect and to decapitated animals. We do not as yet understand all the causes which may affect these movements, nor can we thoroughly explain to what extent or in what manner even the known causes may influence or affect one another. To this conclusion the late experiments of Dr. Volkmann, appear also to have led. "The will," he infers, "prevents reflex motions, because of two opposing forces, the weaker must yield, and the power of the will is often sufficient to keep muscular motions in control. It is not asserted, however, that decapitation favours the production of reflex motions solely by the removal of the mental influence: on the contrary, it appears probable that there are other, but unknown causes engaged; for the inclination of the nervous action to cross from filament to filament is so great in decapitated amphibia, that it would require a very uncommon degree of mental influence to control them, on the assumption that it was the only controlling power."

But if, after all, there be a doubt that the movements of decapitated animals present as characteristic indications of sensation or suffering as the movements of perfect ones, it is fully compensated for by their obvious adaptation to a determinate end, and their frequent spontaniety, both of which phenomena are wholly inexplicable if the existence of sensation be rejected. Dr. Volkmann states, that a decapitated tortoise when irritated, conceals itself beneath its shell, and a decapitated frog comports itself according to the nature and degree of the irritation. When its

fore feet are irritated, it withdraws them; when further irritated, it withdraws them further; and when yet further irritated, it draws them in below the belly, and changes the sitting for the recumbent position. When the posterior extremity is violently irritated whilst it is in the sitting position, it will bound forwards; when roughly seized in the thoracic region, it will plant its fore feet upon the hand which holds it, *and try to free itself;* and when the skin of the abdomen or back is seized with a forceps, it is by no means uncommon for the mutilated animal *to scratch the part with the posterior extremity of the corresponding side.* Mr. Grainger states that, "upon irritating the cloaca in a green frog which had been decapitated, the most violent motions were excited in the hind legs, *and repeated attempts were made by the limbs to remove the instrument with which the cloaca was touched.* This fact he has since repeatedly seen in the green and common frog, both when the head was removed, and when the spinal cord was divided in the back; and he also noticed it in the common fly and other insects after decapitation. He has observed, too, that if, after having cut off the head in frogs, fire is applied to the fore part of the trunk, *violent motions to remove the source of excitement are made.*" It is, however, needless to multiply facts of this nature; they are now familiar to every vivisector, and prove most fully that the motions which result from irritation in decapitated animals possess all the characters of adaptation to a determinated end.

Dr. Marshall Hall, however, denying the sentient nature of the spinal cord, will not admit that either ordinary indications of pain, or the adaptation of movements to a determinate end, are any undoubted evidence of its existence. Spontaniety of action alone, he admits as the true test of sensibility; and this, with consciousness, he believes disappears after the removal of the brain, chiefly because decapitated animals, when once they become quiet, remain immoveable in their assumed position if not irritated, as long as life remains. This latter (if the fact,) would tend to prove absence of thought and volition rather than sensibility; but the experiments of numerous physiologists are directly opposed to it. Decapitated frogs, after remaining at perfect rest for some moments, are known to assume the sitting posture. In the experiments of Fleurens and Magendie in which the brain was removed, the animals, when pushed forward, continued to move after the

impelling force must have been wholly expended. Mr. Grainger found, on pricking the feet of the posterior half of the body of a salamander, which had been cut into two pieces, that the motions of the limbs and tail which followed were several times repeated, from the one application of the stimulus; and Dr. Marshall Hall states that a Coluber natrix, whose spinal marrow was divided between the second and third vertebræ, continued moving for a long time when once irritated, the movements at last gradually subsiding. Both Mr. Grainger and Dr. Hall explain these apparently spontaneous actions not as the result of sensations but of impressions, their continuance or repetition arising from new parts of the limbs or surface of the animals coming in contact with the table or ground at every altering position, and so occasioning new reflex movements. On this supposition, however, it would be more difficult to understand why the motions should cease at all, than why they commenced, at least as long as any power to move, or excitability remained. It would also be difficult to explain their ceasing gradually, as one might anticipate they would do if dependent on sensation, instead of suddenly, as a movement originating in mere impression might be expected to do; and it remains to be shown why, in the headless frog, the contact of new parts with the ground, when it assumed the sitting position, and felt, as it would seem, comfortable, occasioned no new movements. Professor Volkmann, who has been the latest experimenter on this subject, was so struck with the sentient nature of the movements, that he believed the decapitated animal was aware of the nature of the stimulus, and chose from a variety of means those which were best calculated to relieve it. Although I cannot go this length, I quite agree with the professor, that the reasons Dr. Hall offers for supposing the movements dependant on impressions, and not upon sensations, are very unsatisfactory. Dr. Hall observes, they not unfrequently cease when the body is in a very uncomfortable or even painful position, if it retain sensation: they ceased, for instance, in a serpent whilst the tail was hanging over the sharp edge of a table, and were not renewed by pricking it, or burning it with the flame of a candle; but, as Dr. Volkmann remarks, their non-occurrence here was owing to the exhaustion of the excitability; and this is obvious, because pricking and burning in general produce reflex motions in decapitated animals.

In drawing inferences from such experiments as are here referred to, it is essential to recollect that the phenomena of sensation, especially those which include the combined and harmonious action of many muscles in one movement, can only be expected to take place where the organic conditions essential to the action of the cord are preserved, which they can seldom be for many minutes after decapitation in the more perfect animals. Hence it is that in amphibia, reptiles, and cold-blooded animals, they are found more energetic and lasting. In the classes of yet more simple structure, whose nervous systems consist merely of double cords without brain, the sentient or reflex phenomena are still more remarkable, for another reason. In proportion as animals are endowed with the higher powers of the mind—memory, consciousness, and perception—the instinctive and purely sentient actions are more feebly developed, and the dominion of pure volition more extended; but as the mental powers decrease, the actions of the muscles are less confided to volition, until they end altogether in dependence upon instinctive or sentient influences, where the cerebral portion of the nervous system entirely disappears. The action of the spinal cord therefore in man and the higher animals, after the removal of the brain, even if the conditions essential to the healthy performance of its functions were maintained, must necessarily be at all times less remarkable, and partake less of spontaniety or design, than in the segment of a polypus which was never endowed with any higher organization than a double nervous cord.

It will be observed that I have here spoken of sensation (feeling) and sentient action as independent of consciousness and volition in the popular sense in which these terms are made use of, guided in my inferences by pathological facts and anatomical experiments, without making any attempt to reconcile these with received metaphysical definitions of the mental faculties. In all animals, certain organs bearing more or less likeness to one another in the several classes, are essential to the performance of certain functions; and it seemed to me a fair inference, whenever an organ was wanting and no substitute could be discovered (supposing the animal large enough to admit of examination,) that its functions were wanting also; as when no organs of vision can be found, it is justly concluded the animal is blind; and where no organ of hearing is discoverable, deafness is inferred.

Thus, as indications of sensation in organized beings are co-existent with a nervous system, and are obviously dependent on it, the legitimate consequence of its absence in any organization would appear to be insensibility; and as we have the strongest evidence that the higher faculties of the mind, such as are essential to consciousness, belong to the cerebral hemispheres, whenever these organs are absent or destroyed, we unhesitatingly conclude that consciousness is absent or destroyed also, Conversely, we must infer, if certain faculties seem unimpaired after the destruction of particular organs, those faculties must belong to some of the organs which yet remain; and why such inference should not be considered as absolute as any of the former in establishing sensation or feeling as a property or function of the spinal cord, which so many experiments have proved the unimpaired existence of, after the removal of the brain, it is difficult to say; unless the scepticism on the subject depends, as I believe it does, on erroneous notions of both consciousness and sensation derived from abstract reasonings on the mental faculties. Before we can advance one step further in this inquiry, it is essential to shew that these erroneous notions do actually exist, and have been universally received by psycologists from the remotest period to the present hour.

It is perhaps true, that we can arrive at a knowledge of the human mind only by reflection on its acts and operations; but this can be admitted solely on the understanding, that the inferences from these reflections are strictly tested by a comparison with the inferences from physiological investigations; and I question much whether, in any case, the metaphysician would not reason more securely if he first investigated accurately the construction and properties of the several nervous masses or organs with which the faculties of the mind are known to hold connexion, and so laid the foundations of his future and more abstract inquiries, in anatomical and physiological facts, which no new views or discoveries could alter.

The very vague manner in which the terms mind, consciousness, perception, volition, and sensation, are employed by almost all writers, would sufficiently convince one of the truth of this, if it were not otherwise obvious. The term mind is sometimes used to express the understanding, thoughts, or intellectual functions only; sometimes these and the sentient, and sometimes the senti-

ent alone, as when pure feeling or sensation is spoken of. Consciousness is used almost indifferently for perception or sensation, and these latter often as indifferently for one another. The term volition is applied with equal uncertainty, half the disputes relating to the voluntary or involuntary character of muscular action having arisen from a mere disagreement as to what volition means. Let us consider what we are to understand by these several terms, and first—of consciousness and volition.

SECTION II.

The term consciousness has been used by metaphysicians in two senses: the first an exceedingly limited one, implying merely the existence of a sensation, thought, or desire—a pure sense in fact of being or of existence; the second, or true and popular sense of the word, "a belief in the existence of the sensations and thoughts which pass through our minds, and of our own existence as the subject of them."* In the former sense it is simply another term for sensibility, and is used indifferently for it by Brown, who denies its existence as a distinct faculty. He considers consciousness merely as a general term for sensation, thoughts, or desires, and identifies it with all or any of these, inasmuch as it is, to use his own words, "impossible to feel and not to feel at the same time; by which he means, that it is impossible to feel and not be self-conscious at the same time. Now this is the very point at is sue; for if there be a class of pure sentient as distinguished from thinking beings, which we shall shew is more than probable, the one can only possess a sense of feeling of existence or pure sensations, while the other experiences conjointly with these sensations a perception of them, with a belief in their own existence as the subject of both. No general term, therefore, applying to the two definitions indifferently, can be admitted in physiological reasoning without leading into continual error; so discarding the use of the word consciousness in the first sense attached to it, expressed much more simply by sensibility or feeling, we shall employ it only in that full meaning in which it is legitimately and popularly understood.

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* Alison's Physiology. p. 215.

It will appear that consciousness, in the full sense in which we have defined it, of necessity implies the existence of memory and personal identity. It is difficult to understand how even an intuitive belief in our own existence, as the subject of thoughts and sensations, could exist or occur in any way to the mind, until at least a second sensation or thought was experienced, and remembered as having been experienced, by one and the same being. The mind cannot dwell, even for a moment, in thought or inference on its own existence, without including the existence of memory and individuality, since the occurrence of the thought or inference must have arisen from a previous thought or sensation remembered as having been experienced by the same being. Pure sentient beings, if such exist, may have a sense of existence ; and this is all, I believe, the infant has in the first moments of life, or can have until it is capable of perceiving its own sensations. Mental consciousness, as distinguished from mere sentient consciousness, sensibility, or feeling, therefore implies, not only the perception of thoughts and sensations, but the reference of these to something that remembers the experience of a former thought or sensation, which believes it existed before the present moment, and that it was itself which experienced all.

The distinctions here drawn between sensation and consciousness have always been recognized by metaphysicians, though these continually confound or identify them in their application. Brown who altogether rejects them, is indeed consistent in using the terms indiscriminately, yet he sometimes used the word consciousness in a more extended sense, not simply implying a mere sensation as the consciousness of the moment, but a series of feelings or sensations. "If the mind of man," he says, "and all the changes which take place in it, from the first feeling with which life commenced to the last with whioh it closes, could be made visible to any other thinking being, *a certain series of feelings alone,* that is to say, a certain number of successive states of the mind, would be distinguishable in it, forming, indeed, a variety of sensations, and thoughts, and passions, as momentary states of the mind, but all of them existing individually and successively to each other." To this whole series he gives the name of consciousness.

Now consciousness, as has been shewn, in its very essence implies individuality, and we can conceive its divisibility as little as

we could the divisibility of mind, or the thinking principle ; while if Brown's definition were admitted, and that we could suppose it to consist in a long series of feelings, without any connecting link or subject, and without any necessary or obvious relation to one another, there is no difficulty in conceiving the divisibility of an animal with such an amount of consciousness, each segment retaining the same consciousness as the whole. That consciousness which consists in a mere sense of the present, neither including remembrance of the past nor anticipation of the future, is not of necessity indivisible, or attached to a single existence, as mental consciousness must be ; but the consciousness which implies sense and perception of the present, with memory of the past, includes all the individuality which mind comprehends, and to suppose the possibility of its division would be, in fact, to suppose not only the possibility of a single mind becoming two minds, and capable of existing in different states at the same moment, but of each mind remembering the past as wholly experienced by itself. Consciousness, therefore, in Brown's exposition of it, is nothing but pure sensation, or what is popularly termed feeling, without thought, or memory, or knowledge, or belief of its own existence ; while, in its ordinary and proper acceptation, it implies not only a knowledge or belief of the existence of the sensations felt, but of the being who experiences them.*

All that can be essential to consciousness, thought, perception, memory, belief, we have already seen, are connected with the

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* It is singular that Brown, while denying the distinctions here insisted on, points to them incidentally in his reasonings on various occasions. It would be difficult to draw a clearer picture of *a sentient being*, as compared with *a percipient one*, than we find in the following extract :—"Even if by some provision of nature our bodily constitution had been so framed as to require no subsistence, *or if instinctively and without reflection* we had been led, on the first impulse of appetite to repair our daily waste, and to shelter ourselves from the various causes of injury to which we are exposed, though our animal life might have been extended to as long a period as at present ; *still, if but a succession of momentary sensations*, it would have been one of the lowest forms of mere animal life. It is only as capable of looking *before and behind*—that is to say, as capable of those spontaneous suggestions of thought which constitute *remembrance and foresight*—that we rise to the dignity of intellectual beings." In common with other metaphysicians Brown also frequently uses the terms *sentient mind* and percipient mind, as if he meant two essentially distinct and independent faculties.

cerebral hemispheres, and perish with them; volition, therefore, as far as it is understood to be a purely mental act, must also be considered as strictly dependent on the same organs. Like consciousness, it has been used very vaguely, some applying it to all actions of the muscles which are under the control of the will; others limiting it to those actions in which there exists the consciousness of a mental effort. The latter limitation is obviously founded on a strict sense of the popular understanding of the term, as well as its essential relation with those faculties which exist in connexion with the cerebral lobes. The actions which take place after the destruction of those lobes, and in connexion with another part of the nervous system—the spinal cord—however they may evince proofs of design in the adaptation of means to ends, can be looked upon as instinctive or sentient only (if, indeed, the instinctive and sentient functions survive the destruction of the brain); and before even any reasoning can be admitted in support of an opposite conjecture, it must be experimentally demonstrated that the faculties essential to consciousness, and therefore to volition, have no necessary dependence on the brain; which I do not believe can be done.

We have, indeed, abundant proofs, both analogical and direct, that the cerebral lobes are esssential to the existence of consciousness; and yet, since all the phenomena usually considered indicative of it—motions expressive of volition, of suffering, of design, and even spontaneous actions—appear to survive the destruction of those lobes, it follows at the least, from all we have stated, that our sole means of determining whether certain muscular movements are the result of consciousness and volition, or of an instinctive sentient faculty, must rest in the definitions which we attach to the words, and in the anatomical relations of such faculties as are essential to the fulfilment of those definitions. This is, indeed, the only true mode of arriving at just notions of the physiology of the brain, or of the mental functions; and it is because it has never been adopted by physiologists that the received doctrine of mind cannot by possibility be reconciled with the new facts daily arising from the rapid improvements in physiology. "All that we know of the body," says Reid, "is owing to anatomical dissection and observation; and it must be by an anatomy of the mind that we can discover its powers and principles," as if those powers or principles, studied without reference to the organs with

which they are connected in life, could be correctly or with certainty distinguished and understood, any more than the functions of assimilation without reference to the organs concerned in them. To assume the examination of mind distinct from body, is to examine the operation of our own minds distinct from the conditions under which only we can be conscious of them: it is as if we attempted to attain a knowledge of the visual powers by studying our perceptions of objects, without reference to the organs which alone can furnish the conditions under which vision takes place. The liability to error in inquiries so conducted is well illustrated by the difficulties in which the philosophy of mind has been involved by modern discoveries, which seem to have almost demonstrated that thought and feeling reside not only in different, but in distant parts of the nervous system, and that the latter may exist independently of the former. This will appear still more clearly in some experiments, which, returning to our inquiry regarding the organs concerned in consciousness and volition, we shall now instance. It will be seen that they place metaphysicians on either horn of a dilemma, leaving them to declare that consciousness admits of division, and of existing as so many new and perfect consciousnesses or minds as there are divisions, or that all the phenomena which have been hitherto considered essentially dependent on, and characteristic of consciousness, may exist without it.

No one, with the notions of consciousness and sensation at present universally received, will deny their possession to an earth-worm: its sensibility to irritants—the voluntary nature of its movements—the freedom with which it appears to choose its means of escaping from danger—the certainty that it hungers and thirsts and has sexual appetites, all seem corroborative of the fact. From the simplicity of its organization, too, and the extent to which it admits of being divided into segments, each segment still retaining the conditions essential to its organization, it appeared to present the fairest subject for experiments regarding conciousness that could be selected. These experiments were found to furnish the following results:

An earth-worm was suddenly divided into nearly equal parts. The anterior part moved rapidly away without much apparent suffering; the posterior writhed itself violently as if in great pain; but after a few movements lay quiet. It continued so for about

a minute, and then moved on with the wounded end foremost. progressing at first slowly, but afterwards quite as actively as the anterior part. It sometimes selected a course to the right, sometimes to the left, without any obvious cause. Occasionally it rested, remaining quite motionless, so that I thought it would not move again. In some short time, however, and without the application of any stimulus, it moved onward spontaneously again. There was, indeed, no possibility of distinguishing it from the anterior part except one looked sufficiently close to detect the wounded termination of the former, so different from the pointed head of the latter. At one time, when progressing steadily on, it was observed to stop suddenly to evacuate, as a cart horse does on a journey, and after very deliberately discharging the contents of the bowels it moved on again.

Another earth-worm was divided also in the middle; the fore part moved rapidly away; the tail part writhed as before, then remained quiet for half a minute or more; after which, without the application of any irritant, it moved on like the anterior part. When it came in contact with a pin stuck perpendicularly in its way it moved to one side; when two pins were stuck before it so close as scarcely to allow room for passing, it got apparently jammed between them, after which it shewed no disposition to move, until stimulated.

When the same experiment was tried with the tail part of another earth-worm, allowing more room between the pins, it first came directly against one of the pins, then turned a little and began to pass it; but immediately after, as if changing its mind, drew back and passed forward between the two pins. When a penknife was placed crossways in its path it first stopped, then turned its wounded part to the right and moved along the surface of the knife in a direction at a right angle with its former course.

Another earth-worm, nearly three inches in length, was divided with a sharp scissors about three quarters of an inch from the head. The anterior part, instead of running rapidly off, as in the former casses, contracted itself suddenly and lay still, while the posterior darted away tail foremost across the table, as if in excessive alarm. When it had traversed a good distance, fearing it would fall off the table, to the edge of which it was approaching it was touched at the tail (the foremost point as it progressed) with a probe, upon which it instantly reversed its movements,

taking a directly opposite direction without turning, the wounded part being foremost. The anterior part of the worm all this while lay very quiet, but it now began to lift its nose and snuff about, turning its pointed head now to the right, now to the left, yet still retaining its place, so that I began to think it was either glued to the table by the moisture, or had lost the parts of its body necessary to progression. On putting a little bit of earth near it, it coiled round it, and remained so. The posterior part of the worm was meantime continuing its exertions, when suddenly, and without any external cause that I could possibly divine, the anterior piece set out on its travels. As it happened to take the direction of the posterior piece, it seemed almost as if it was going in search of it. The posterior part had by this time got to the edge of the table, upon which I watched its movements with great curiosity. It fitst stretched its foremost or wounded part out beyond the table, to look for support or something to cling to; but not finding anything within reach it turned it in, and fixing it to the perpendicular end of the table, about half an inch or more from the upper flat surface, it arched its body as a leach would until the tail nearly reached the margin of the table, when losing its hold, it fell to the ground. On being taken up it was still lively and inclined to move, but I took no further notice of it.

Another earth-worm was divided into three parts; about half of it having been left to the centre portion, and a fourth each to the head and tail. The two latter writhed a good deal after the division and then moved on slowly and feebly. The centre piece moved on more actively, changing its direction to right or left, and avoiding obstacles as the former segments did.*

Satisfactorily as the experiments on the higher or more perfect animals must be deemed, in proving the persistence of the phenomena which are considered indicative of consciousness after the

* In several such experiments I found that when the worm was divided into three portions, the motions of each are more languid and feeble than when divided into two; and when divided into two unequal ones it is feeblest in the shortest piece, whether it be the anterior or posterior. When divided in the middle, the anterior part is always at first the more active of the two, and usually moves on without much appearance of pain, while the posterior one writhes and seems to suffer though it eventually moves on also. When the posterior part is the longest it sometimes hurries off without any convulsion or writhing, the suffering in such case appearing only in the smaller segment left behind.

removal of the brain, they were still open to objections on the part of those who consider the results as wholly opposed to the old and acknowledged doctrines. But here is an animal, the earth-worm, to whom consciousness is fully conceded, and to whom, if it was not conceded, it would be idle to argue on the subject, since no other principle could be shewn, by those who identify consciousness and sensation, as capable of producing the same phenomena; here, we say, is an animal, each segment of which, after division, enjoys the same apparent consciousness and volition as the entire; highly sensible, moving about freely as feeling or caprice prompts it, resting when tired, travelling on when rested, avoiding obstacles, endeavouring to overcome difficulties, and all spontaneously. It may be fairly inquired, how will this be accounted for by the metaphysician? It will not answer to say, as has frequently been said, that in the lower orders of animals (as the invertebrata) the sensorial property becomes less and less concentrated in single masses, and the character of individuality ceases to attach to the sensorial phenomena. As a statement of a fact it may be undeniable, but if intended as an explanation it must be regarded as absurd mystification on the part of those who cannot conceive the existence of any degree of sensation without consciousness. If such animals enjoy sensation at all in any shape or degree, they must enjoy consciousness according to the received doctrines, and consciousness, even in the lowest degreee which the metaphysician attaches to it, includes individuality as much as it does in man. Consciousness means self-consciousness if it means anything. It is, in fact, the thinking principle attending to its own acts,* and can be conceived no more divisible as such, into two perfect and independent centres of consciousness in the lowest than in the most perfect organization.

How then are we to regard these extraordinary phenomena? how are we to explain these facts, of animals exhibiting all the indications of consciousness and volition, yet surviving the destruction of the organs on which true consciousness is dependent? If we again revert to the experiments which raised this difficulty, and endeavour to ascertain whether they can suggest any clue that may lead us out of it, we find, that with the removal of the brain all the strictly mental faculties which animals possess—memory,

* "Consciousness, appears to mean simply the act of attending to what is passing in the mind."—*Abercrombie.*

perception, and the association of thoughts, wholly disappear; while life and all the usual indications of sensation, and all the instinctive acts most closely linked with sensation remain. The creature swallows what is put into its mouth, moves its legs when irritated, and its wings when thrown into the air; but, when not excited by any impression made on the senses, appears in a state of stupor or profound sleep, gives no signs of recollection, even of sensations just felt, nor of such emotions as sensations were wont to excite; and cannot seek its food, nor even avoid obstacles thrown in its way.

If we were not prepossessed with invincible notions of the identity of sensation with the thinking principle, or of its being simply a condition of mind, it would at once occur to us from these facts, that there was an essential difference between sensation and perception—between feeling and self-consciousness; and that although it might be, that every thing present to the senses, or felt, must also be perceived by all animals having a percipient organ or brain, it does not follow that such animals as are deprived of it, or who never were possessed of one, should not feel. It would, I think, occur to us, that there may be large classes of purely sentient animals as there are of percipient ones, and that in the diseases or accidents to which even the highest organizations are subject, the independent existence of both descriptions of being is illustrated. In apoplexy, in epilepsy, and in profound sleep, in the acephalous fœtus, and in decapitated animals, we have a tolerably perfect illustration of pure sentient existence; and if it be thought so difficult to conceive sensation without perception or consciousness, that we are fain to admit the presence of the latter in those cases, it should be recollected that it is still harder to conceive the presence of perception without a percipient organ, which we have the strongest grounds for believing the brain to be, or the divisibility of consciousness, such as appears to take place on dividing an earth-worm or a polypus into segments.

The difficulty one at first feels in conceiving sensation distinct from perception arises from the fact just stated, that sensation is almost always necessarily perceived in percipient beings, and when not perceived is not noted or remembered by the mind, and so cannot become a subject for observation or argument. Sensation and perception seem thus to be co-existent, and become in some

sort identified. It is a singular fact, however, that invariably as this identity is assumed alike by the uninformed and the philosoper, both acknowledge distinctions and frequently apply these terms differently, and in senses that would not admit of their being exchanged for one another. There is, indeed, no work treating of the mental faculties in which we do not meet with distinct definitions of sensation, perception, and consciousness, and long chapters especially devoted to each, although we are afterwards, in their application, certain to find the identity, of at least the two former assumed.

Dr. Brown defines sensation to be the simple impression made upon the organs of sense; perception, an association formed between this impression and an external substance, which we have ascertained to be concerned in producing it. Dr. Abercrombie describes sensation as implying the corporeal part, and perception the mental part, of the process by which we acquire a knowledge of external things. Reid applies the term sensation to the changes in a part of the nervous system, called the sensorium, consequent on an impression made on the organ of sense, by something external to it; and the word perception he applies to the knowledge of the presence and the qualities of that something following the sensation. "The impression," he says, "made upon the organ, nerves, and brain, is followed by a sensation, and last of all this sensation is followed by the perception of the object."

The distinction will be understood more readily by the simple illustration given by Mr. Mayo. "I look," he says, "at an object of such dimensions that a single glance serves to satisfy me of its nature: the impressions which I receive through this experiment are three-fold:—1st, a present sensation of colour; 2d, a conviction that the sensation is excited by something external; 3d, a notion of the true size, and form, and distance of the object, which I have seen. The first of these impressions constitutes pure sensation; the second, or the notion which we form of something external as the cause of sensation, constitutes perception; the third class, of impressions described, and which we have learned to associate with the preceding, are our acquired perceptions."

What we give the name of perception to in common, it is true, is always made up of impressions, sensations, and perceptions, the latter including the two former; but when we come to examine

and define them more particularly, as we have here done, it does not appear that the existence of the former so necessarily includes the latter. If, indeed, there be any difference at all inferred in the definitions given, it must be admitted that every successive action in the series forming perfect perception may exist, though not followed by that which should naturally succeed it: thus the first impression on the nerves or brain may take place without being followed by that change in the sensorium called sensation, as when a limb which has lost its sensation is irritated, or an amaurotic eye exposed to the light. Again, sensation may take place without being succeeded by perception, as we have reason to believe occurs in apoplectics—in the acephalous fœtus, and perhaps in newborn infants—in decapitated animals, and in the segments of those animals which admit of division, without destruction of the organization essential to the life of each segment. Lastly, such instinctive perception may take place without the acquired, of the truth of which we have no need of illustration.

SECTION III.

In prosecuting an inquiry of this kind, the physiologist no doubt finds himself continually staggered in the most apparently legitimate inductions by the recurring question, "Am I then to believe that an animal may experience a sensation, suffer absolute pain, without knowledge or consciousness of it?" Yet this is not more incomprehensible than other metaphysical truths when first proposed to the mind. The young philosopher is quite as much perplexed when he is first taught that all his knowledge of an external world, of solidity, and extension, is not derived purely from the sense of touch; nor his knowledge of figure, magnitude, or distance, from the sense of vision. Brown has, I think very truly observed, that every original and accurate analysis of our sensations *must afford a result* that will appear paradoxical. Yet in this, as in every other correct analysis, the paradox begins to disappear in proportion as we learn to think clearly on the subject. For instance, when once we can conceive, even remotely, the possibility of a sentient existence as distinguished from a percipient one, it is not difficult to comprehend, that an animal to whom we deny consciousness of being, may yet have a sense of being; and,

in like manner, that although it may have no consciousness of pain, it may have a sense of pain.

The dogmatic manner in which metaphysicians have generally rejected the distinctions between sensation and perception or consciousness here suggested, even those persons who themselves applied the terms differently, and treated of them separately, was little to be wondered at before the extraordinary discoveries in the anatomy and physiology of the nervous system made of late years. But it certainly is calculated to excite some surprise that Mr Grainger, who has lately written a very ingenious and interesting work on the functions of the spinal cord, and than whom no one was more fully informed of the apparently insuperable difficulties connected with the old opinions on the subject, should venture to dismiss the question in the same absolute and unargumentative manner. "It is, indeed," he says, "apparent, that in every systematic treatise the most obscure statements prevail respecting the nature of sensation. Physiologists, although on these points they always speak in a most vague manner, seem to imagine that there are, in fact, two kinds of sensation—one which is attended with perception and consciousness, and one which is not; *but it implies an absurdity to admit a sensation which is not perceived.* In the case of vision, for example, the light falls upon the retina, and produces, by its contact, what is called an impression; *but this impression is not converted into a sensation until it has been perceived by the mind.*" Again, he says, "If we free ourselves for an instant of all these confused notions of physiological writers, it becomes evident that there is but one kind of sensibility, *for the very term sensation implies something of which the mind is conscious;* thus, for instance, if I touch a piece of wood with the finger, a certain effect is produced on the ends of the sentient nerves, which is called an impression; and this impression, when it has been transmitted to the brain, is by the agency of that organ perceived, *and then, but not till then, sensation is produced.*" In these extracts all the assertions (given in italics) are mere gratuitous assumptions of the very points at issue. Not to go any further, if, as Mr. Grainger states, when perception takes place, and not till then, sensation is produced, we may fairly inquire what earthly difference he makes between sensation and perception. If impressions produce no sensation until perception takes place, it must be something subsequent to, and arising out of, the percep-

tion not antecedent to it, as is usually presumed* ; or it must be synonymous with it, and so superfluous. In such case, indeed, much needless difficulty and error would be avoided by always speaking of impressions and perceptions only. But it will be at once seen, by the observant reader, that the opinions Mr. Grainger considers so erroneous have arisen as legitimate inductions from facts and experiments, which, if not satisfactory, are at least more so than any others one can arrive at; while the assumed absurdity of these opinions is derived either from his own theoretic or preconceived notions. For example :—When once it was ascertained that sensation, to all appearance, remained uninjured after the destruction of the brain—the acknowledged organ of consciousness—it became evident that the sentient portion of our being—the sensorium—was very different from, and independent of, the conscious or mental portion, and that sensation could be no longer looked upon as a mere function of mind; yet Mr. Grainger asserts, as if it were almost a self-evident proposition, " that the very term sensation implies something of which the mind is conscious ;" and upon this assumption proceeds unhesitatingly to strengthen the foundation of a far more difficult and unsatisfactory theory—that of Dr. M. Hall on the reflex function.

Considering, however, the very perplexed and vague notions of most physiologists on these subjects, it was perhaps not unnatural that those who found it impossible to conceive the independent existence of sensation and consciousness should seize with avidity upon the only other conjecture that made any approach to a solution of their difficulties. Extravagant as the notion might appear that the attempts made by a brainless or headless animal to remove a source of irritation with its paws, just as if it had not been mutilated—that the cries uttered by it when pinched, or when a hair of its whisker is pulled, as in the rabbit—or that its dartings forward and endeavours to escape, should be considered as mere insentient phenomena, offering no indications of suffering, and produced by an independent reflex action of the cord; it seemed to present less difficulties than the more commonly received doctrine, that these cries and movements were voluntary, and yet persisted after the

* Reid says, " The impression made upon the organ, nerves, and brain, is followed by a sensation, and *last of all*, this sensation is followed by the perception of the object."

destruction of the only acknowledged organ of consciousness and volition—the brain ; or that in the lower orders of animals, contrary to all analogy, consciousness and volition were transferred from the brain (the organ with which they are known to be connected in man) to the spinal cord. From all that has been already stated, it must occur to the reader that the perplexities in every view of the subject with modern physiologists, have arisen from the vague and unmeaning manner in which the word volition has been applied. It is understood, and properly understood, to imply mental effort; and so long as it was spoken of in connexion with consciousness or a percipient organ, the application was intelligible ; but when the brain was gone, and, of course, perception and thought with it, volition was still spoken off in connexion with sensation. The motions which persisted after decapitation, it was said, were the result of sensation and volition; or, in other words, they were the result of a mental act, after the organ was annihilated, in which it was acknowledged the animal's mind or perception, such as it might be, resided. If the plain and obvious induction which the results of pathological observation as well as of experiments on living animals suggested, that consciousness and volition—the ascertained functions of the brain—were extinguished with it; yet that as the signs of suffering, the motions invariably resulting from pain persisted after that extinction, sensation must necessarily persist also as their efficient cause; almost all the difficulties which forced Dr. M. Hall into a new, and, I think, unsatisfactory hypothesis, would have disappeared. The actions of animals after decapitation would then have been attributed to the same influence —sensation, to which so many complicated muscular movements are attributed in the perfect or unmutilated animal, and they would have been denominated not *voluntary*, but *sentient* action ; they would have been classed with the actions of respiration, coughing, sneezing, sucking, yawning, vomiting, &c.—with the motions of the apoplectic, of the acephalous infant, and with many of the movements performed in the state of reverie, or in profound sleep.

The extensive influence of sensation in all our muscular movements is so masked by the association of the will in percipient beings, that it is difficult to estimate it. Not only are the actions of respiration, coughing, sneezing, &c., wholly independent of the will, but even the artificial combinations of muscular actions

created by it, could never be accomplished without the aid of sensation, and are, it would seem, eventually placed almost entirely under its dominion. As a very simple, yet striking instance of the manner in which mere sentient actions are overlooked in percipient beings, and attributed to volition, I may mention the case of a patient whose arm was paralyzed. By the utmost efforts of his will, made at my desire, he could not move it more than five or six inches from his body, and could not raise it at all; yet, whenever he was seized with a fit of yawning, his mother assured me both arms were extended, and raised freely above the head, but on no other occasions. The movements in yawning have always been considered voluntary, yet here all the efforts of the will were unable to accomplish what sensation accomplished easily and perfectly, proving that those movements are really sentient and not voluntary.

That such movements are not the result of the reflex function any more than of volition, but are dependent on sensation, I cannot offer stronger proofs than those proposed by Whitt and others, so admirably stated by Professor Alison, and so little affected by any arguments which Dr. Marshall Hall and Mr, Grainger have been since enabled to bring forward. Professor Alison states—

1st. That sensation, as a cause, is adequate to the effects ascribed to it, is manifest from the fact, that the changes in involuntary muscles, and in secretions thus ascribed to sensations, are not only closely analogous to, but in several instances identical with, those which are allowed on all hands to be excited by emotions.

2nd. That all such phenomena as we ascribe to sensation, coexist in the animal system only with indications of sensation.

3rd. That, in various instances, sensations may be excited by impressions made on *different* and distant parts of the body, and the actions which succeed them in these different cases are the same, which proves that these actions follow, not an impression or irritation of any particular nerve or organ, *but the excitation of a particular sensation.* This is illustrated by the very various modes in which the sensation of nausea, and the complex effects succeeding it, may be excited, and by the excitation of laughter, on tickling different and distant parts of the surface.

4th. Conversely, when different impressions made on the *same* part of the body, excite *different* sensations, even al-

though it be certain they are felt through the medium of the same nerve, they are not followed by the same action. Thus, of many sensations felt through the first nerve, very few only are followed by any diminution of the heart's action, or by retching.

5thly. When the mind is much engrossed, either by previous sensations or by interesting trains of thought, any impressions on the organs of sense are transiently and imperfectly felt; and in these circumstances it is observed that the actions in distant parts, usually following such impressions, are either suspended or imperfectly produced—a further proof that the intervention of the sensation is essential to their production.

6thly. The remedies found to be most effectual in stopping or preventing these actions when in excess, are either such as make strong and new impressions on the organs of sense, thereby diminishing the effect of sensations already existing, or else such as blunt the sensibility in general, and so diminish all effects of sensation.

The incontrovertible inferences from these, as well as from the many other facts already considered, were gradually leading physiologists to the conclusions respecting sensation and consciousness which it is the object of the present pages to establish. It was beginning to be very generally acknowledged that the complicated actions of muscles in respiration, sneezing, coughing, vomiting, &c. were independent of the will; and that these, as well as most of those apparently spontaneous movements occurring in decapitated animals, were, however inexplicable it might be, the result of a persisting sensibility. The adherents of older opinions had retired from the field, their doctrines in no sense enabling them to offer an explanation of such extraordinary phenomena, when a new theory was proposed—new, not as regarded the fact of movements arising from a reflex action of the spinal cord, but of that reflex action being excited by the impressions simply, and not by sensations.

It is not my intention here to enter into a particuiar examination of the doctrine of the reflex function, as proposed by Dr. Marshall Hall, but merely to point to such general and important objections as seem at the very outset difficult or impossible to get over.

The theory of combined muscular actions excited by impressions on the extremities of nerves, without the intervention of sensations, necessarily supposes definite and direct connexions be-

tween the excitor and the reflex nerves in all cases. Yet Professor Alison has fully shown that no anatomical discovery—no conjectural connexion of the nerves in their course or at their roots—can account for the muscular actions which arise in obedience to certain irritations applied; these actions accompanying or succeeding one another in great variety, not according to the parts or nerves irritated, but according to the sensations excited.

It supposes the reflex action to be produced by the transmission of impressions through the excitor or incident nervest distinct from those of common sensation, and not connected with the brain, to reflex or motor nerves. This, is admitted, would oblige us to believe in the existence of a separate incident nerve, connected, by means of a particular point of the spinal marrow, with several reflex nerves for every different combination of muscular action occasioned by irritation of any single part of the frame. It would oblige us to believe that in the fifth pair are bound up, besides the pure sentient nerves, one nerve for the reception of impressions of cold applied to the face, and connected with the phrenic and intercostal, by which the act of respiration is performed; and also with the cutaneous nerves of the whole surface, by which a general constriction of the capillaries is produced; another for the transmission of irritations of the nostrils, connected with the phrenic and lower intercostals and lumbar nerves, by which the act of sneezing is performed; and so a new and distinct nerve for every new modification or combination of muscular action which can arise from varying irritations applied to it. The energy or extent of a reflex action may vary with the degree of irritation applied, although the nature of the combination or of the sympathetic actions cannot vary, unless by the excitement of a new sensation.

To quote, again, Professor Alison's observations on Sir C. Bell's respiratory system of nerves, which apply as fully to Dr. Marshall Hall's theory, "It offers no explanation whatsoever why irritation in the fifth pair in the nose excites sneezing, while irritation of the same nerve in the cheek excites full inspiration only; and other violent irritations of the same nerve and same branches of it, excite no respiratory action whatever—why the nerves of the diaphragm and abdominal muscles act simultaneously in the actions of vomiting and of straining, but alternately in the actions of coughing, sneezing, laughing, weeping, &c.—or why the nerves of

the diaphragm act alone in the case of full inspiration, whether from irritation of the fifth pair in the face or of the par vagum in the lungs—or why the nerves of the abdominal muscles associate themselves and act simultaneously with those of the muscles of the face in the actions of sneezing, laughing, weeping, but not in those of full expiration or coughing, or tenesmus, in which they themselves act with equal force—or why the laryngeal branch of the par vagum, which moves the arytenoid muscles, acts in con cert with the nerves of the abdominal muscles, and closes the glo-tis in the actions of vomiting and tenesmus, but escapes such combination in the actions of laughing, coughing, or sneezing."

The theory assumes that sensation is only temporary or incident, and not an essential in these actions; but it is not proved that any of those reflex movements attributed to sensation can take place without its intervention, and seems probable only in very few.

It includes a rejection of the evidences of animal suffering which have been acknowledged by mankind in all ages, and which are assumed instinctively by the brute. It compels us to believe, that when mutilated animals are writhing in torture, their cries, their struggles, their well directed and natural efforts to remove the instrument of torture, or to escape from it, are mere automatic movements, wholly unconnected with pain. It would, indeed, take away from every human being the proofs upon which the sufferings of his own fellow-creatures is inferred, beyond the mere assertion on their parts, if it was not that the natural signs of strongly felt emotions or sensations affect us instinctively, and far too powerfully to have their influence supplanted by the most specious reasoning.

Finally, if it were even admitted that the struggles and cries, and natural efforts of brainless animals to escape from torture, afforded no indications of sensation, it is not denied that the persistence of spontaneous motion is an incontrovertible proof that sensation still remains: in fact, spontaniety of action is proclaimed, by Dr. Marshall Hall, as the distinguishing character of the sentient and voluntary system, as compared with the excitomotor. He says, unhesitatingly, "these latter are never spontaneous; they are always excited:" and, in illustrating the fact by division of the spinal cord in a frog, observes, "I divide the spinal marrow below the occiput, with these scissors: all is

still. Not a trace of spontaneous motion. The animal would remain in this very form and position, without change, until all signs of vitality were extinct." I have already pointed out that Dr. Volkmann has found this assertion of Dr. M. Hall's to be erroneous. "If, after the first convulsive movements following decapitation in the frog have subsided, and when the body is little irritable, it be extended on a hard surface, it will (he asserts) retain the position in which it has been put for five or ten minutes; when suddenly, and without the application of any external stimulus, it will draw up its thigh, and change the recumbent for the sitting position." But it is unnecessary to attempt determining this point by reference to experiments in which there can be a possibility of doubt, when we have the fact already stated, of the divided earth-worm, before us. Fleuren's chicken, which lived ten months after its brains were taken out, it is said, never moved except it was irritated; but this is precisely what might be anticipated in an animal so high in the scale of being as to have its ordinary movements more dependent upon perception and volition than upon instinct or sensation, In a purely sentient animal, however—in an animal without brain, or with a brain to which the sentient cord is little subservient—division of the body does not deprive the segments of the conditions essential to spontaneous motion. Its movements in the perfect state being the necessary result, not of perceptions and volitions, but of sensations; and their nature being determined by the quality of those sensations, there is little injury occasioned by division, except perhaps the destruction of some particular instinct, which, unlike common sensation in such animals, may be connected with some limited point of its organization.

After all that has been here objected to the doctrine of reflex motions from mere impressions, it is not my intention to deny the existence of such motions in any case. The contractions of the iris on the admission of light, of the lids in winking, and of the uterus after death, in those cases in which labour is said to have been completed in the coffin, are sufficient to prove that sensation is not, in all cases, essential to the accomplishment of the action which usually succeeds it. In all instances, however, in which reflex movements follow mere impressions, it will, I presume, be found that the connexions between the incident and reflex nerves are clear and direct, as in the several beautiful illustrations of

such connexions given by Sir Charles Bell. The relations of muscular motion with the nervous influence, are, I believe, still far from being clearly defined or understood; and there are yet many complications to be unravelled by the physiologist, before he can arrive at the simplicity of nature's arrangement. The most numerous and complicated movements of the living frame, appear to be the results of sensations or volitions: some few, classifiable, no doubt, under some simple law, arise from mere impressions; and some, as the movements of flexion and extension, may perhaps be explained by the doctrines of antagonism suggested by Bellingeri.

SECTION IV.

If it were impossible for me to offer the slightest explanation of the opinion that sensation(by which I mean feeling, in the popular sense of the term,) is different from, and may exist independent of perception, consciousness, or knowledge—if the division of a sentient being into two separate independent existences was as inconceivable as the division of a conscious or percipient one, I should scarcely believe myself the less bound to admit the clear inferences deducible from the extraordinary facts stated in the course of this inquiry. It has been incontrovertibly determined that perception, thought, memory, consciousness, and the intellectual functions, are connected with the cerebral lobes, and disappear with their removal. Yet after their removal, and when no parts are left above the corpora quadrigemina, and in amphibia and cold-blooded animals even after decapitation, signs of sensation and suffering, and even spontaneous actions, are observed to take place, to doubt the reality and dependence of which, would be to discredit the instincts and intuitions that make part of our very nature.

I cannot but think that sufficient has been already said to convince the reader, if not of the absolute existence of the distinctions proposed between sensation and perception, at least that those distinctions are conceivable. That they are so I can give no stronger proof than is offered by Reid, in his metaphysical speculations on the subject. "If nature," he says, "had given us nothing more than impressions made on the body, and sensations in

our minds corresponding to them, we should in that case have been merely sentient, but not percipient beings. We should never have been able to form a conception of any external object, far less a belief of its existence. Our sensations have no resemblance to external objects; nor can we discover by our reason any necessary connexion between the existence of the former and that of the latter. We might perhaps have been made of such a constitution as to have our present perception of external objects, without either impressions upon the organs of sense or sensation. Or, lastly, the perceptions we have might have been immediately connected with the impressions upon our organs without any intervention of sensations. This last seems really to be the case in one instance—to wit, in our perception of the visible figure of bodies."

It did not occur to Reid how truly he was depicting the character of a large class of animals in these speculations of what he conceived as merely possible. The animal creation may perhaps be properly considered as including three classes of organized beings. Those with insentient or ganglionic nerves only, and whose organs perform their functions without feeling or consciousness on the part of the individuals, as the heart and bowels do in man —if, indeed, any such simple organizations exist even in the lowest type of animals.

Those, again, described as possible forms of existence by Reid —the pure sentient organizations—animals possessed of a spinal cord, evincing obvious signs of sensation, and displaying actions evidently resulting from and dependent on such sensation—animals possessing appetites and instincts, and apparently sensible to pleasure and to pain—animals whose lives are but chains of momentary feelings, successive but independent of one another, who, possessing no faculty of perception or memory, have no consciousness either of their own existence, or of the existence of the world about them, and who, having a simple organization, throughout which a single sense is equally diffused, admit of being multiplied by division into more numerous existences.

And finally, those to whose spinal cord a brain is superadded, to whose sentient existence a percipient and conscious one is attached, whose life is not made up, as in the sentient being, of a thousand separate existences—a thousand transitory sensations, which, though succeeding each other in one organization, come

into being and die without any absolute relation; but is impressed with a sense of continuity and individuality by new and extraordinary faculties, which perceive and detain and record every fleeting sensation, and which can recal, and examine, and compare, and dismiss them at pleasure. It may be said, indeed, that the life even of a mere sentient being has a sort of continuity; the series of sensations of which it is made up are connected; it can hardly be called related, by its organization; and there is no period, however short, while that organization holds perfect, in which sensations are not experienced. But it is an animal living always strictly in the present; no moment of its existence having a conscious relation with the past or future, any more than if each single one belonged to a distinct and different individual. The life of the animal with brain, on the other hand—of the perceptive conscious animal, is connected from its earliest to its latest period in all its pains and pleasures, by a memory which recals them at a moment, and an intuitive conviction that they have been experienced by the same single being which recals them.

In this view it may be said, that in all the higher classes of animals, the organization is made up of three distinct forms or types of existence, each wholly independent of that which has succeeded or is superadded to it. It is no slight proof of the truth of this hypothesis, if such it may be called, that the independence of these three distinct modes of life may be illustrated by actual analysis, as Magendie, Fleurens, and Bouillaud, have shewn; that the percipient organ may be destroyed, leaving the sentient and ganglionic organization uninjured, and the sentient may be destroyed, leaving the insensient or purely vital functions actively proceeding; that, in fact, as an animal is built up, so may we contrive carefully to unbuild him again.

Take away, for instance, the anterior lobes of the brain, or let there be an apoplectic attack, and the conscious percipient portions of the being are extinguished, while the purely sentient and organic remain. This may, perhaps, be held as mere assumption with regard to the apoplectic; since, if signs of sensation are evinced, there are grounds for supposing that some degree of consciousness remains: but in the case of ablation of the brain, or of the brain never having been formed, as in the animals experimented on by M. Magendie and Desmoulins, and in the acephalous

fœtus, there can be no doubt as to the absence of consciousness, unless we doubt that the brain is the organ upon which that faculty is dependent. Yet signs, apparently the most indisputable, of the persistence of sensation, are freely displayed. "When, after having opened the cranium, all the parts of the cerebrum, the optic lobes, and the entire cerebellum, are cut away, the animal continues sensible to all the impressions which have their seat in the face, excepting that of sight. It continues to be as vividly affected by sounds, odours, sapid substances, and punctures of the face, as if it experienced no other inconvenience than that which results from the mere loss of blood. It cries if a hair of its whisker be plucked, or if a strong acid be applied to the nose, and endeavours, with the paws, to remove any source of irritation as it would do if it had not been mutilated."*

It may be naturally asked, when the phenomena so generally attributed to perception and volition are so strikingly exhibited in animals mutilated in the manner described, why we should deny them these faculties, or look upon them as no longer conscious but sentient beings? Such a question may, perhaps, be most aptly answered by another. Supposing the definitions of perception and volition, of sensation and sentient action, already offered, to be correct, what phenomena should we expect to disappear on removal of the conscious or percipient organ? The animal should lose its memory, its powers of association, its recognition of objects, its knowledge of, or connexion with an external world, its incentives to action. If it felt hunger, or thirst, or cold, it would lie still and suffer, having no perception either of its wants or the means of relieving them. It would hence remain inert, and as if in profound stupor, unless excited by the stimulus of physical agents in contact with it, or by such causes as usually call up instinctive muscular movements. It would, on stimulation, display all such actions as are most closely linked with pure sensation or instinct; the amount or extent of such actions being very much determined by the amount or degree in which the conditions essential to the performance of the functions of the spinal cord are interfered with in each particular animal, by the mutilation or decapitation. In the cold-blooded animals, we know them to be interfered with very little, and in the earthworm and poly-

* Anat. Des Syst. Nerv, p. 560.

pus, which have no brain, and in whom instincts stand in the place of perceptions and volitions, still less.

Let us now compare these supposed results with those which have been found actually to take place on ablation of the brain in animals which have survived the operation, I cannot refer to a more striking or satisfactory experiment than that of M. Fleurens on the chicken, already mentioned, which lived ten months in perfect health, after the operation.

"He had scarcely removed the two cerebral lobes before the sight of both eyes was suddenly lost; the hearing was also gone, and the animal did not give the slightest sign of volition, but kept himself perfectly upright upon his legs, and walked when he was irritated or when he was pushed; when thrown into the air he flew, and swallowed water when it was poured into his beak.

"He never moved unless when irritated; when placed upon his feet he remained upon them; when resting on his belly, in the manner chickens rest when asleep, he appeared plunged in a sort of drowsiness, which neither sound nor light in the slightest degree disturbed. Nothing but direct irritation, such as pinching, or pricking, or striking, had any effect in rousing him.

"When the animal did move about, it seemed to do so without any motive or object, though there were no convulsions nor any want of harmony in its movements; if it met with any obstruction, it did not know how to avoid it.

"The chicken was quite healthy, and, five months after the operation, the wound had quite healed, and a new layer of bony matter was forming.

"Still it had no sense of smell or taste; neither had it any sensation of hunger or thirst; for, after allowing it to fast for three whole days, and then placing food immediately under its nostrils, and afterwards putting it into his beak, and putting its beak into water, it did not shew the slightest disposition to eat or drink; and would have died for want of nourishment, if it had not been fed by force.

"It seemed entirely to have lost its memory; for if struck against any body it would not avoid it, but repeat the blow immediately."

M. Fleurens, Dr. Marshall Hall, and Mr. Grainger, are of opinion, that this chicken, during the long period of ten months

which it had survived the operation, never experienced one single sensation or perception—that it was wholly insentient, and all its movements automatic; yet it walked and flew, and swallowed and drank, upon occasion, and aroused itself from its usual stupor when pinched, pricked, or beaten.

Looking upon the animal, however, as having retained common sensation, how perfectly explicable are all the phenomina detailed; how strictly they accord with those which might be naturally anticipated from the mutilation.

Sight and hearing were lost. These functions, though perhaps like common sensation dependent on the spinal cord, are to any useful purpose essentially associated with the perceptive faculty, and incapable of being effectively exercised without it. It does not at all follow, because sensation is presumed to be distinct and independent of perception, that all the peculiar senses were calculated to be of use, or intended for existence independent of it. For almost all useful purposes of the sense of sight, and perhaps of smell and hearing, perception is as essential as memory.

There is so much of experience and rational induction required in all the perceptions by these senses to make them serve any useful purpose, that the impressions they convey disconnected with such inductions tend to no end. Hence it may perhaps be considered as a universal fact, that all animals possessing special senses, such as man possesses, must also have a percipient organ or brain. This I believe will be found to be the case with respect to all animals possessing the sense of sight, the most intellectual of the senses; and of this we may be at all events assured, that if any animals without brain possess organs of sight, they can only derive from them the mere sensation of light.

The chicken did not shew the slightest sign of volition (spontaneous motion,) and never moved unless when irritated. As a purely sentient animal, so rendered by the removal of its percipient organ, it would of course evince no volition; but it was so far inferior to a much lower order of beings—those which were created purely sentient—that it had not, in the absence of a percipient organ, the sentient instincts which in such animals supply the want of motives and volitions; nor could its sensations so readily take the place of volitions, and spontaneously suggest or occasion movements unless aroused to action by external irritation.

When it did move about, it seemed to do so without motive or object; and if it met with any obstruction, did not know how to avoid it. Here, again, we see the disadvantage the chicken laboured under in not having been created without brain; for if it had, all, even the least of its sensations would have led to the necessary results as instinctively as those painful feelings did which made it walk when struck, and fly when thrown into the air, and swallow when food was put down its throat. It had lost the perception and memory which heretofore enabled it to avoid obstacles, and was gifted with no sentient instinct that could answer as a substitute.

It neither hungered nor thirsted. This perhaps is very doubtful. Hunger and thirst are pure sensations, not necessarily connected with cerebral perceptions in any way, and capable of being as universally diffused as common sensation throughout the entire frame, if nature had so pleased it. One should not, therefore, at once anticipate the loss of such sensations on ablation of the brain, though it is probable, in the higher classes of animals, that these as well as other peculiar sensations having their seat so high up in the spinal cord, are wholly extinguished by decapitation. If the chicken both hungered and thirsted, I think the observable phenomena would have been still the same. Having lost its memory, and with it, its perceptions—its associations—in short, all its knowledge or consciousness of an external world—food might be placed under its nostrils or in its beak, and its beak into water, and yet it would evince no greater disposition to eat or drink than if a piece of iron was offerred to it for refection. It may be, and I question whether it is not the case, that, in animals created without brain, the simple sensations involve immediately in themselves some obscure feelings which must be wholly superfluous in animals created with a perceptive organ through the instrumentality of which their appetites are supplied and hence it is, such animals fall below the merest sentient being when deprived of that organ.

The acephalous infant furnishes us with an illustration of a purely sentient existence, still more satisfactory than that of M. Fleurens' chicken. Headless infants, or at least infants possessing merely the basis of the skull and face, and devoid of the brain, have been known to live for some days after birth. In these cases, sucking, deglutition, respiration, and the expulsion of the

urine and fæces, were performed as in the perfect state, *cries were also uttered*, and the motions of the limbs usual with infants after birth took place. Such motions, however, after a little time occurred only on excitation by some external agent. Are we to believe, that there was nothing whatsoever of sensation or human feeling in the phenomena displayed during the short lives of these little beings?

Mr. Travers mentions a case in which the os frontis was driven in by a fall, and a considerable portion of the brain extruded at the wound of the scalp; yet the boy, "although utterly deprived of consciousness, made obvious but unavailing efforts to aid the surgeon in getting him into bed, and thrust his arm mechanically into the sleeve of a clean shirt, after his hand had been placed in the opening of the sleeve."

If we can implicitly admit the correctness of Mr. Travers' opinion, that in this case there was an utter absence of consciousness, as I presume we may, the phenomena are quite inexplicable on any other hypothesis but that which assumes the integrity of the sentient system after the destruction of the conscious or percipient. The motions performed it will be observed, were neither voluntary, that is, designed, nor instinctive, as the actions of sucking or yawning. They were the result of trained associations, that is, of actions artificially associated with particular sensations. Mere impressions may possibly excite certain combined actions, the connexion being established from the first in the organization; but no connexion can be conceived between arbitrary actions associated with our sensations, and mere impressions.

These are all instances of purely sentient life persisting after ablation of the percipient or cerebral life. But if the spinal cord —the organ upon which all sensation in living beings would seem to be dependent, was destroyed as well as the brain, in the gradual manner adopted in Dr. W. Philip's experiments, we find a lower grade of life, the organic, yet remaining—we find the heart and vessels of circulation persisting in their functions, and even the secretions going on for a period. Sensation is now utterly extinct. Muscles may contract on stimulation as long as their irritability lasts, but there are no longer evinced any regular or associated movements, any expressions of suffering, or phenomena indicative of design, or spontaneity of action. This last and

lowest organization, forming the basis on which the sentient and percipient systems are erected in the higher order of animals, may perhaps be itself like each of these, the type of an immense class; yet there are too many strong analogies in favour of the more popular notion, that feeling characterizes even the lowest forms of animal existence, to permit our coming to any decided conclusion on the subject.

In this manner, by removing the superstructures successively, we may unbuild the frame of the perfect animal, so as eventually to reduce it to the simplest organization—the lowest of living forms. But we have other very striking evidences, that the organic, sentient, and percipient forms of life are essentially distinct, in the discoveries of Tiedemann and others respecting the developement of the fœtus. Tiedemann has shewn that in the earliest period of fœtal life, the brain and spinal cord do not exist, while the organic functions are in active operation. At the end of the second month the spinal cord and two anterior prolongations or peduncles of the brain are formed, or in other words the sentient existence is established. Finally, the cerebral lobes become gradually developed, and the organization essential to thought and memory and consciousness is achieved.*

It is singularly illustrative of the distinctions here pointed out, that they are observable in the dying animal as in the process of its formation. Each form of life dies, too, precisely in the order one might anticipate from its known dependencies. The organic, the first formed, and the basis upon which sensation and perception subsist, necessarily lives longest; while the percipient system, the last added, and the least essential to existence, dies earliest. It is extinguished indeed temporarily in very slight disturbances of

* "It is a singular but beautiful poysiological fact, that living organizations generally, should be developed and called into being, should obey the same laws and rules of development, should become more complex and perfect in the same gradual manner in which man individually, from the first germ of his existence to his perfect form, exhibits; that the animal kingdom, from its minutest microscopic point of vital endowment, should pursue an analogous perfection of development of different organs, should exhibit the same systems mutually dependent or rising into or out of each other, that are called into operation in rapid succession during the short nine months of embrytic and fœtal existence; that we ourselves in embryo should lead a similar aquatic life to many of these animals!"—*Anderson on the Nervous System.*

the organic or sentient functions—in common fainting, in apoplexy, and in epileptic fits, and even in profound sleep we are as wholly unconscious of existence as if the percipient organ was removed. In dying too, the loss of mental consciousness always precedes insensibility, though sometimes perhaps only for a very short period. The time for which the sentient life may persist after the loss of the percipient is very uncertain, though generally very inconsiderable in the higher classes of animals, and differing much in the warm, in the cold-blooded, and in the amphibia. The diseases of the human frame furnish us with numerous instances of the continuance of sentient life, in cases where we could have no doubt there was a suspension of all consciousness—an extinction, for the time, of knowledge, thought, and memory; and even when perfect death is impending, and sensation itself has expired throughout the rest of the system, it still clings to the respiratory function—the last and most important with which it is connected.

I very much doubt whether the fœtus in utero at any period, or even the infant of a few hours old, is a percipient being—that is, a being exercising its percipient organ; nor, if I am correct in my conclusions, can there be a more beautiful example of pure sentient existence than the latter presents as it rests or sleeps for the first time in the lap of its nurse. Never having, as I presume yet experienced a distinct perception—a sensation referred to the external cause which produced it—it cannot have thought, or knowledge, or belief, or memory of anything. It feels and moves; it withdraws its hand if touched, it turns, or throws its arm outside the clothes, or draws up its little knees and seeks an easier position to lie in; but it refers no sensation to a self that felt that or other sensations before. It has perhaps a constant sense of existence, such as may possibly be inseparable from sensation; arising not from any knowledge, or belief, or thought about itself as distinct from an external world, nor from any reference of that sense to a thinking being which experiences it; but from whatever instinctive feeling of individuality may be imparted by a constant succession of sensations in one organization. All men during profound sleep are in fact, like the acephalous or the new-born infant, but mere sentient beings, and the many successive sensations with their associated muscular movements which make up the night, the turns and changes of position, unattended by any after-consciousness, and which are usually attributed to

unremembered volitions, have, I believe, if the truth could be strictly ascertained, never been perceived.

To recur again to the fœtus in utero: as the mother is sensible of its movements after the fourth month, it is obvious its limbs are obedient to sensations before the brain is at all properly developed, and therefore before mind begins. The flexion and extension of the limbs, the kicking and elbowing which every mother knows are performed with considerable energy before birth, cannot be fairly attributed to volition, unless the existence of perceptions and thoughts are at least presumable, Even after the child is born, it can only will those motions which are already obedient to sensations, as flexion, extension of the limbs, &c. It cannot originate a motion not connected with or trained to act with a sensation; it cannot, in short, accomplish a very unnatural or unusual movement. The first actions of the new-born infant are thus purely sentient, and have no more connexion with volition than those other actions which are the acknowledged results of instincts or sensations, as sucking, sneezing coughing, respiring, &c. As it gets perceptions and ideas, its actions become gradually associated with volitions; but those volitions are still for a time confined to the instinctive or sentient movements; they then become connected with sensations excited by seeing actions performed before us and lead to imitative movements: last of all they become associated with perceptions of a more abstract nature, and follow patiently the thoughts of the mind.

It has been already observed, that purely sentient animals are not endowed with the higher of the special senses, whose exercise to any useful end requires the assistance of a percipient faculty. Common sensation which they enjoy, and which is generally diffused throughout the entire frame, seems capable of accomplishing all that is requisite to life, at least in that very lowest form of animal existence which betrays no mark by which it can be recognized, beyond a diffused sensibility. If a percipient faculty was not essential to the exercise of the higher functions of vision and of hearing, there is no reason why these senses should have been confined to minute organs, or rather why they should not have been diffused throughout the frame like common sensation, so that sights and sounds and external feelings should be appreciated in common by the cutaneous nerves on all parts of the sur-

face, as the odours of the rose, and musk, and valerian are by the olefactory nerves.

There is no fact which more forcibly demonstrates the distinction between sensation and perception, than the vast distance observable between animals endowed merely with a diffused sensibility, and those possessed of such special senses as require the the assistance of the lowest degree of the percipient faculty. Even multiplying the higher senses, or rendering them more acute, does not necessarily raise an animal in the scale of being, any more than multiplying or improving its instincts would. But rendering its perceptions permanent, and increasing its power of abstracting and comparing, add considerably to the perfection of its understanding. Some of the lower animals are said to possess a sixth and even a seventh sense, besides possessing those in a much greater degree of perfection which they enjoy in common with us; yet their vast inferiority would scarcely be lessened in the comparison were two of our special senses to be taken away.

It must readily occur to the reader, from all that has been stated, how much the confusion in which metaphysicians have involved themselves about the nature of sensation and perception, must have tended to perplexity and error in their general estimate of affections of the mind. It seems strange, for instance, that what are called external affections of the mind—those sensations which relate to, and connect us with, the world about us—should be classed with internal affections—those perceptions, thoughts, and memories, of which our consciousness is made up, and which we feel might remain, though our external relations were altogether changed. What is there, we may ask, in sensation, in bodily enjoyment, or in suffering, or in pleasure, or in pain? what is there even in our instincts or appetites, which all depend upon, or hold connexion with the world we live in, and our relations to one another, that should induce us to identify them with the percipient and conscious mind? that mind which sees no essential or everlasting affinity between its thoughts and the things about it—which can conceive new conditions, and accommodate itself to new relations —and which acknowledges no necessity for its death, though the world were extinguished?

I am, after all, most ready to admit, that even in the present advanced state of physiological science, it is difficult to explain or

discuss all the phenomena connected with sentient and percipient existence very minutely, without danger of committing some metaphysical absurdity. It may be, that every feeling includes some obscure intuitive sense of being; it may be, that perception, instead of being a distinct faculty, as Reid and others considered it, may be, as Brown conceived, "nothing more than a suggestion of memory or association, which differs in no respect from other suggestions, arising from other co-existences or successions of feelings equally uniform or frequent;" but, however these points may be eventually decided, it cannot affect the conclusions which have been followed out in these papers, so long as it is undeniable that evidences of sensation are displayed after ablation of the brain, but none of either perception or memory. The exposition of the greatest errors, or even of the most absurd reasoning, if such were detected, would yet go but a little way to shake the foundations upon which the opinions here advocated have been based, so long as it is unexplained how an animal possessing sensation, evincing spontaniety of movement, and evidently enjoying the pleasure of alacrity of action, or of ease, or of gratifying its appetites, admits of subdivision by the knife, each part displaying the same phenomena which previously pertained to the whole? So long as it is unexplained how the brainless rabbit and the headless frog evince the ordinary movements of suffering when tortured, and make the ordinary efforts at escape; and, finally, so long as we remain unconvinced that, when cries are uttered by animals wanting the cerebral lobe, they experience no pain.

It has been said, it is true, that the experiments on living animals, from which these facts are deduced, are not to be trusted, and that the inferences from them are even contradicted by pathological experience.* But no pathological experience or discovery can make it untrue, that movements to an end are made and cries uttered, by the acephalous infant, or the brainless rabbit; or that

* I have no doubt that when we acquire habits of observing the physiological symptoms arising from diseases of particular parts of the nervous system more accurately, we shall find our experience daily corroborate the results of experiments carefully performed on living animals. The rotatory motions, the retrograde and the motions forward which have been observed by Magendie in his experiments on the influence of the cerebellum on muscular motion, have been since noticed as direct results of diseases in animals; and I have myself seen, within the last month, an instance of disease within the ear, in which the symptoms

signs of suffering are evinced, and efforts to free itself displayed by the headless frog. It has been said, on the other hand, by those who deny neither the facts nor the inferences deducible from them, and as if it offered a satisfactory explanation, "that, although in man the sensorium is exclusively confined to the brain, as we descend in the scale of being, it becomes more extended. That in some amphibia we may conjecture that the spinal cord partakes with the brain in all its faculties, and, as we advance to animals that have a still simpler organization, the brain entirely disappears, and the spinal cord appears to be substituted in its place."

A little consideration must convince any physiologist how very untenable are these assumptions of a diffusion or transfer of a function or a faculty from one organ to another. No one was ever bold enough to imagine the functions of the liver or spleen could be transferred or extended to the kidneys, or to any other wholly dissimilar viscus; and, surely, in the shape and arrangement and anatomy of the brain, not to dwell on any other distinctions, there is as great dissimilarity to the spinal cord displayed as could exist between any of the viscera which differ most widely in function. When we cannot detect organs of vision or of hearing in any animal, we do not straightway suppose that these senses are transferred to other parts, but rather that they are wholly wanting. When the brain has been removed, or in cases where it never existed, why, therefore, should we not at once determine that the functions of that organ, whatever they might be, are wanting also? Or why should we rather venture a conjecture that in any, even the lowest form of vertebrated animals, the spinal cord becomes a substitute for the brain? "The investigations of comparative anatomy," says Mr. Grainger, "and the laborious

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strikingly resembled the effects occasioned by injuries of portions of that organ in the experiments of Fleurens. This celebrated physiologist found on dividing the semicircular canals of the ear in birds in certain directions, rotatory motions to the right or left, or motions of the head up or down were produced. In the instance of disease of the ear alluded to, in which a discharge from the organ had existed from childhood, the patient informed me, that whenever he pressed his thumb firmly on the mastoid process, he immediately spun round, and unless he removed the finger fell to the ground.

inquiries into the process of developement, have led to the establishment of one grand principle in the science of organization—the unity of structure. There is no truth in any branch of human knowledge fixed on a more firm basis than this; that although nature displays immense fertility in varying and modifying the form, and other physical characters, of the several organs, yet that there is in no one instance a departure from the first original type. This, the great law of the organic world, has been demonstrated in the osseous, nervous, glandular, and other systems, with amazing exactness; and as a consequence, it has also been discovered, that the human body from the period of its first appearance as a semi-fluid and shapeless mass, till it attains its perfect formation, passes through many stages, in which its several organs temporarily assume the permanent structure of the lower animals. There is thus a chain of evidence, in which no link is wanting to prove what are and what are not analogous parts. Notwithstanding the complete establishment of this most important principle of the science of organization, it is still maintained that properties, that is to say, sensation (perception) and volition —which are known to be the special attributes of certain parts of the human brain, and which parts, although developed in an inferior degree, have still a real existence in the lower animals—become translated in these instances to another independent organ, possessing its own peculiar endowments—the spinal cord. If such a mode of deducing facts be recognized in physiology; if, in fact, the function of one organ may thus be transferred to another and a distinct structure, merely to reconcile the anomalies of a crude and ill-supported hypothesis, all the great truths of modern anatomy have been discovered in vain."

PROBLEM X.

BY W. GRIFFIN, M. D.

WHAT ARE THE THERAPEUTIC EFFECTS OF OPIUM?

I BELIEVE it may be said without exaggeration, that in the whole range of the Materia Medica there is no medicine more valuable than opium, whether considered with regard to its power of arresting alarming disease, or of alleviating suffering. It is not for us in the present age, daily accustomed to witness its wonders as ordinary effects of an ordinary remedy, to estimate the miseries which poor human nature must have endured in the want of it anterior to the times of Hippocrates, when its properties were unknown. Those only can fully appreciate its influence, who have for the first time witnessed its power of allaying pain with the suddenness of a charm, or of summoning sleep to the pillow as if at the word of an enchanter. It stands out, too, in strong relief amidst the multitude of remedies which having acquired temporary celebrity, were again shorn of their pretensions, or sunk into absolute oblivion; for it has been daily acquiring a greater and firmer reputation, and scarcely a year passes by in which some new application of its extraordinary powers is not brought to light by the observant industry of the profession.

It may perhaps appear a very needless task in these days to enter into any discussion on the effects of a remedy of such acknowledged popularity, and in such extensive use. But it will always be found that remedies of extraordinary power, require a proportionate care and judgment in their exhibition, and that the facility with which they may be abused, bears a just proportion to the benefit derived from their happy application. It appeared to me, too, that in some very important points, as regards its appropriate doses or combinations with other medicines, in the treatment of particular conditions of disease, medical men are not so

fully agreed as would be desirable, and hence, to the young practitioner at least, the result of many years experience on the subject may be interesting.

One of the most ordinary applications of opium, is, for the purpose of procuring sleep, and it is well known that it effects this object with far more certainty than any other narcotic. Particular idiosyncrasies of constitution are however occasionally met with, in which, so far from composing or evincing its usual hypnotic powers, it occasions sickness of stomach, or retching, or, brings on a disturbed sleep with distracting dreams and affrighted startings, or occasions a sinking and faintness, which alarm the friends or attendants of the patient. In these instances we are commonly warned to avoid opiates altogether, and certainly in most instances we may act most judiciously in doing so. It sometimes happens however, even in these cases, that some pressing symptom will demand an opiate, and it becomes most desirble that we should in some way counteract its apprehended ill effects. When these effects are confined to sickness of stomach and retching, I have seen them forstalled and prevented by the addition of some powerful aromatic (say one or two grains of Capsicum,) to the opiate. It is much more difficult to qualify the remedy in cases where it is likely to disturb and excite the whole nervous system, from some idiosyncracy of habit; but when this can be accomplished at all, it will probably be done best by combining Camphor, or Ipecacuanha, or emetic Tartar, in certain proportions, with the Opium or Morphia. When the results are sinking and faintness without stupor, they commonly arise very many hours after the exhibition of the medicine, and are in fact, mere symptoms of exhaustion, following the declining influence of an over dose. They may be removed or alleviated by renewing the opiate in a smaller dose, or, if other considerations render this unadvisable, by stimulants frequently repeated, and eventually by sleep. It is often of great importance in medical practice, and especially so in all dangerous cases where opiates may have been largely administered, that we should remember a period of exhaustion and sinking is likely to come on from twelve to twenty-four hours after the opiates have been discontinued. It almost invariably occurs, where the opiates have previously been long continued, or taken in frequently repeated doses, or where there is a great constitutional susceptibility to their action. I

knew a lady, who for some chronic illness, having been obliged to take a great deal of Morphia, gradually diminished the quantity after her perfect recovery, until she reduced the dose to some very small amount taken twice in the day. Her first dose was usually taken at noon, but one day having forgotten it, she fainted on the sofa in about an hour after her time for taking it had gone by. Her imagination had so little influence in producing the effect, that she did not recollect the omission of the Morphia until some hours after, and did not confidently attribute her fainting to it, until convinced of the connexion by its repeated occurrence under similar circumstances. A medical friend of mine, who took large doses of opium, for several days, for chronic rheumatism, told me if he had not experienced it, he could have formed no conception of the degree of exhaustion and depression which came on when he allowed the usual period to go by without taking his dose. All this would lead one to infer the necessity in cases where opium has been largely or repeatedly exhibited, to let the influence of the medicine decline gradually by giving diminished doses at long intervals, but, I advert to the subject chiefly on account of the danger or difficulty which often arises, when the physician either forgets the quantity of opium he has been giving, or is not sufficiently alive to the probable effects of its sudden withdrawal. Finding his patient sunk, his countenance pallid and depressed, and his whole appearance betraying, sometimes, signs of excessive debility, he attributes the change to some unfavourable turn in the course of the disease, becomes alarmed for his safety, and, perhaps, resorts to medical measures either unnecessary or mischievous. This is particularly apt to occur in the cases of young children or infants, when the violence of the inflammatory symptoms has gone by, and it has been found necessary to administer opiates. About the time the influence of the opiate is wearing out, if it has been a large one, the eyes look sunk, the eyelids lie half open, disclosing a small portion of the white cornea, the face is deadly pale, the skin clammy, and the whole appearance of the child suggests an apprehension that it is dying. Yet if a little nourishment, or some slight stimulant be given, or even if a little time be allowed to elapse, the heart will recover its tone, the little patient will revive, and may, perhaps, finally appear to be even in an improved condition.

It has been remarked in almost all essays on the effects of

opium, that an under dose is likely to disagree and occasion a disturbed night, when a full one will give composed sleep. I have never however, seen it stated, that an over dose will produce precisely the same effects, where a moderate one would have given a quiet night, which I believe to be the fact. When an over dose of opium has occasioned a disturbed, though drowsy night, with sudden startings from sleep and rambling dreams, or when even a moderate dose has occasioned those or other unpleasant effects in highly susceptible habits, I have constantly observed the patient obtain a quiet, sound, and refreshing sleep, without any anodyne on the following night. This is precisely as if the somniferous effect of the drug was simply deferred for four and twenty hours, and came on only after the distressing nervous excitement, immediately consequent to its exhibition, had subsided. In giving large and repeated doses of opium in inflammatory diseases, I have, as soon as the inflammation subsided, seen the same consequences take place. While the inflammation was at its height the narcotic power was altogether resisted—when it declined, sleep came on, but if the opium was now continued, either in the same dose, or at the same intervals, the patient was thrown into a state of stupor, or, if repeated in more moderate doses, and at longer intervals, (although, still to an unnecessary amount,) he became restless and passed the night in that troubled, dreamy, and unrefreshing slumber, to which I have already adverted. I am anxious to direct the attention of the profession to this consequence of an over dose particularly, because, I have seen physicians under the circumstance, draw a directly opposite inference from it, and imagine they had given too small a quantity of the drug, when, nevertheless, on being induced to forego its repetition on the ensuing night the patient has enjoyed a long and profound sleep.

I have no observations to offer on the influence of opium, as an anodyne, which are not familiar to medical men. In its administration as such however, one question would suggest itself, which, though of much interest, I have seldom seen adverted to. *When a large dose has been administered, and a patient is still suffering intense pain, how long should one wait before it could be considered safe to repeat it ?* Or in other words, how long may it be before the influence of the drug is felt by the system? I think I may state with confidence, that some effect will be observ-

able from any dose of crude opium, within half an hour, when it is capable of influencing the system at all. The effect of a solution of acetate of Morphia, or of Laudanum, may probably be experienced a few minutes sooner. If, therefore, no obvious effect whatever is produced in alleviating pain or spasm, or inducing drowsiness after the lapse of half an hour, we may safely repeat the dose however frequently it may have been given before. It appears to me so certain, that opium, if given in a sufficient dose to produce any effect upon the system, will evince some sign of its influence in half an hour, I should receive with the utmost distrust any evidence of poisoning by that drug, some symptoms of which did not arise within half an hour after its administration. It is not to be inferred from this, when *moderate* opiates are given for the purpose of producing sleep, that the dose is inefficient if no drowsiness be experienced within half an hour. It is well known the first effect of such doses is almost always excitant and that the disposition to sleep will not come on until such excitement declines. It is, indeed, on this account generally advisable, especially in the treatment of constitutions highly susceptible of the exciting influence of opiates, to give the dose early in the evening, several hours at least, before the time for retiring to rest. As a general rule, it will be found that a large dose of opium produces a sedative effect soon, if capable of producing it at all, while a small dose evinces its sedative influence late, and after an uncertain interval, varying with the constitution and amount of drug administered. The sedative effect which comes on late is never near so dangerous.

One of the most common consequences of the exhibition of large and repeated doses of opium, appearing only after the complaint for the relief of which it was administered has been subdued, is an incapability of passing water. This I believe, arises in all instances from an injurious distension taking place, as a result of the diminished sensibility or irritability of bladder, occasioned by the opium. It is therefore highly necessary for the physician, whenever he is obliged to give opium largely and frequently, to watch the state of the bladder, and if there be any defficiency in the discharge of water or fullness over the pubes, to stimulate that viscus and excite the action of its muscular fibres by full doses of sweet spirit of nitre or some other diuretic. From a

want of vigilance on this point I have seen it requisite to have the water drawn off with the Catheter.

In administering opium, or indeed any narcotic whatsoever for the relief of pain purely nervous, or connected with some condition of mere irritation and periodical in its occurrence, it will be found that a third part of the dose which may be found necessary to give relief in the paroxysm, will prevent it altogether if given in the interval, a little before its accession is expected. That in fact, a moderate dose given in the interval, will sometimes prevent the accession of the fit, when no quantity however great can controul it, after it has once set in.

The administration of opium as an antiphlogistic may be said to be a modern discovery. Its power of arresting and controuling inflammation was first brought prominently before the public by Dr. R. Hamilton of Lyme Regis, who exhibited it in combination with calomel, to which latter medicine however, he attributed the principal influence. After having satisfied himself of the virtue of the combination of the two, he tried opium by itself, and found in many instances when the symptoms failed to give way for some days, they were very soon subdued by adding calomel to the treatment; but Dr. Hamilton was not at all aware of the large amount of opium which should be given, nor the total inefficacy, and even mischief of small doses in the treatment of acute inflammation. We are indebted to the Italian doctrine of contra-stimulus, whether founded in truth or otherwise, to some most important discoveries regarding the effects of medicine in acute diseases.—Firstly, the tolerance of medicines by the constitution in inflammatory disease in larger doses than were ever before ventured upon, and the safety of increasing those doses in proportion to the intensity of the inflammation.—Secondly, the fact that acute inflammations are sometimes controuled by those enormous doses frequently repeated more effectively than by blood-letting, and that they should in all cases, where they can be borne, form at least an adjunct to that valuable remedy.—Thirdly that the beneficial effects of those large doses does not depend, or is not evinced by their ordinary specific action, and that in fact if such ordinary action is induced, it generally interrupts the treatment. If emetic tartar produces constant vomiting or purging, if calomel produces salivation or purging, or if opium produces stupor, *before the inflammatory symptoms have given way*, it at once arrests the whole

plan of cure, and by throwing the physician upon other and much less efficient remedial measures, aggravates the danger of the case. In enteric and peritoneal inflammation, I have been for many years in the habit of confiding almost entirely in the employment of opium in large doses. In robust plethoric habits and indeed in all cases in which it can be well borne, I adopt the ordinary remedy—blood-letting, and then give two or three grains of solid opium as suggested by Dr. Armstrong. If no obvious effect is produced in abatement of the symptoms, a grain is repeated every hour, and after the continuance of this treatment for eight or ten hours, if there be no relief from pain or tendency to sleep, another general bleeding is resorted to, or leeches are applied. The opium is then continued as before, and I have always found the greater the amount of inflammation the larger should be the dose of the opium, as the power of resisting the influence of the drug is just in proportion to the intensity of the inflammatory action. When the latter is not extreme, and when the powers of life are feeble its influence independent of all other treatment is indisputably evinced, as in those extraordinary cases of peritonitis occurring after the operation of paracentesis, or after perforation of the intestines, related by Dr. Stokes,* and it does not appear probable that blood-letting would be necessary in any case, if it were not that in strong habits, and where the inflammation is intense, there exists a resistance to the antiphlogistic power of the opium just proportioned to that intensity. It is in those cases, the addition of calomel to the opinm is so valuable in securing the success of the treatment, and as the combination certainly possesses greater power in controuling inflammatory action, it ought invariably to be preferred either where the simple opium is failing in its effect, or where from the violence of the symptoms, we have any reason to apprehend it may do so. The objection to employing calomel invariably in conjunction with the opium, arises from the probability of its occasionally inducing salivation, which it will sometimes do where it has been used in doses disproportionably large and frequent as regards the symptoms, or where it has been continued after those symptoms have declined. Dr. Hamilton's object in treating inflammatory cases was to excite the action of the salivary glands, having observed that recovery almost always took

* Dublin Medical Journal.

place, where salivation was induced. He even appears surprised that relief of the symptoms occasionally took place, although the mouth was not affected nor any visible evacuation brought on. But it is now well known that the induction of salivation is not at all necessary for the cure of inflammation, and should never be a matter of indifference with the practitioner. It may almost invariably be avoided by suspending the use of the calomel as soon as the symptoms appear to be manifestly giving way, and this may be done with great safety even in cases of extraordinary severity, where we have so powerful an agent as opium to maintain the advantage which has been gained. The majority of acute abdominal inflammations will be found however to yield to opium alone after one or two large blood-lettings, or where there is much debility without any blood-letting at all.

I have in other papers offered so many illustrations of the success of the opium treatment of inflammation that it is unnecessary for me to dwell more particularly on it here. In rheumatic inflammations it is equally available, but as I shall have to speak more at large on that subject in a subsequent essay, I shall not now detain the reader with regard to it. In the acute inflammation of mucous membranes the employment of opium was some years since looked upon as either of very doubtful advantage, or altogether objectionable, but the remarkable cases (published by Dr. Stokes) of inflammation of the mucous membrane of the bowels with exhausting diarrhea, cured by large and frequently repeated doses of opium, as well as the general experience of the profession, has latterly determined the propriety of the practice. The mischiefs arising from the administration of opium in inflammation may always be fairly attributed to the practice of under-dosing. In inflammations of serous membranes which would be rapidly arrested by large doses—small ones incite and tend to increase the mischief—in those of mucous membranes, small opiates do still more injury apparently, because while they are inefficient to diminish or arrest the inflammation, they check or suppress the diarrhea, which however exhausting, is a relief so long as the inflammatory action continues unabated. A large opiate suppresses the inflammatory action, and the diarrhea at the same time, and as a general principle, I think it will be found that the efficiency of its action will be in proportion to the degree of debility present. This would obviously suggest the propriety of abstracting blood

in those cases, where from the violence of the arterial action, the antiphlogistic power of the opium may possibly be resisted, or where on exhibiting it no relief has been attained. For my own part in the treatment of most intestinal inflammations, whether of serous or mucous surfaces, I would confide more in the influence of blood-letting, large opiates and warm poultices to the abdomen than in all the remedies included in our materia medica.

Even in those cases of inflammatory affections of the mucous membranes of the intestines, where blood-letting would not be borne, and where large opiates have failed, certain combinations of the medicine are yet available, and seem in fact to give us all its miraculous influence without those injurious effects which sometimes interfere with and counteract it. I remember a very deplorable case of dysentery, in which the patient suffered almost unendurable agony, and was reduced to the lowest state of wasting and debility, by the constant pain and discharges from the bowels. The evacuations were not facal, but consisted entirely of blood, mixed with jelly, and sometimes purulent looking matter, and they were ejected with a degree of force attended with tormina, which evinced an irritability of the lining membrane, beyond what I have ever witnessed in the disease. No anodyne administered as an enema however small the quantity of starch it was mixed with could be retained for a moment, and even suppositories however carefully introduced, were equally unavailing. Leeches, the warm bath, hot bran poultices, in conjunction with all such internal remedies as are usually found useful in the circumstances had been previously resorted to, and even opium given in the doses of two grains every hour had no perceptible effect, either in allaying the pain or arresting the discharges. In this state of the patient following a suggestion of Dr. Gregory's, I gave three grains of opium with five of Ipecacuanha at a dose, and directed it to be repeated every hour until some relief was experienced, and then continued every second hour until all pain and discharge ceased. The effect was magical ! Before he had the third dose taken my patient was asleep and enjoyed more perfect and enduring relief than he had experienced for weeks. By continuing the dose at longer intervals as the symptoms abated, and without the assistance of any other remedy, as perfect a cure was eventually attained as the circumstances of the complaint admitted. He made a recovery of many months duration, and although it was

but temporary, and he finally died of disease of the mucous membrane, I do not consider it the least detraction from the value of the remedy. In another case to which I was subsequently called attended with equal danger, if not with equal suffering, the treatment was yet more successful, inasmuch as the recovery was quite as speedy, and was permanent. In this instance, so great a proportion of ipecucuanha was not combined with the opium. Three grains of each were given every second hour, and subsequently as the symptoms yielded a grain and a half at longer intervals.

There are some inflammatory affections, to which, after all, it does not appear that opium can be ever advantageously applied. One of its direct and obvious effects given in large doses to healthy persons is to produce congestion of the brain, and probably in the spinal cord. Of this we have sufficient evidence in the apopletic symptoms it occasions—in the relief of these symptoms by large depletion, and again in the recovery of persons dying of hœmorrhage by large doses of it, which I conceive is affected purely by the slight congestion, it is then capable of inducing, restoring the healthy tension of the vessels in the brain, and so preventing syncope and death. When given in acute inflammatory affections of the brain therefore, its tending is to produce a state of circulation in the organ, which must rather aggravate than diminish the mischief going on there. I believe this is true also in inflammatory affections of the spinal cord, since it is only on that presumption, I can at all account for its total failure as a remedial agent in some cases which have fallen under my observation, and in which it got a fair and full trial. One of these is so extraordinary and likely to prove so instructive, I may venture to give it in detail.

John Keogh while standing in the market on a wet day to see some corn weighed, was seized with pain in the hip, knee, and shin to such a degree as to be unable to walk home. On getting to bed, the pain became worse and was excruciating in the shin and ankle. He took salts and had frequent fomentations to the leg; but spent the night in torture, with all the family about him. On examining the limb, I could not detect the slightest tenderness or soreness, either at the lumbar vertebra, or sacrum, or behind the great trochanter, or at the anterior of the hip-joint in the groin; neither did he complain on rough pressure at

the knee, shin or ankle. He could not turn his pelvis, or twist or turn in the bed without suffering indescribable agony, and in consequence lay in one position in the bed for the last 26 hours; he could nevertheless move the thigh on the pelvis, and the leg on the thigh with the most perfect ease. No motion of the limb gave him pain, provided the pelvis was kept perfectly at rest; the pulse and state of skin were perfectly natural, and the tongue clean. As the case appeared to be a kind of rheumatic neuralgia and was unattended by any symptoms of fever or inflammation, I entertained no doubt that it would be subdued by calomel and opium. I therefore gave him two grains of opium, with five of calomel, and five of antimonial powder, and ordered half a drachm of the wine of the seeds of colchicum with one grain of opium to be taken every second hour, until some relief was experienced. On the next morning I found him still in violent agony lying exactly in the same position, and constantly screaming. Vessels of boiling water were smoking by the bed-side and half a dozen attendants were busily engaged in applying flannels wrung out of hot water to the knee, shin, or ankle as he directed, in which work they had been incessantly employed, since my visit on the previous night. The pain frequently shifted to one or other of the parts just mentioned, coming on quite suddenly, and amounting to an excruciating degree while it lasted. His continual exclamations were "Oh, good God, my leg! rub my leg, will ye! my knee now! 'tis there the pain is—my knee I say—the hot flannels again—for the love of God do something to give me ease—Jim, Jim, what are you about? my shin, my shin, man—the flannels to my shin I say—lower down, lower down again—I'll go mad if I don't get some ease doctor; is there nothing to relieve me?" His pulse was all this while natural—his skin cool, and his tongue clean—he had some thirst and the stomach was inclined to be sick, but these symptoms were probably the result of the opiates and the colchicum. The opiates had also confined his bowels, which had not been now moved for two days, and the abdomen was alarmingly distended and tender. A large dose of castor oil and spirit of turpentine, was immediately given, followed by a turpentine injection. A dozen and a half of leeches were also applied behind the great trochanter, and afterwards a blister. The purgative medicines operated freely, and all the distension of abdomen disappeared, but the

pains below the knee were still unabated, and the fomentations demanded as urgently as ever. He subsequently took a grain of opium with two of calomel, and three of antimonial powder every hour throughout the day. When I called late the same night, the room was dim with vapour as I entered, and from the midst of the clouds of steam used in fomenting, which, in some degree, hid the patient and attendants from me, I heard the same incessant commands roared out to one or the other as he wished to have the flannels shifted, when the pain relieved in one place became agonizing in another. This had been going on again the whole day, and not only his own family and relatives were now worn down in succession with watching and fatigue, but such of his good natured neighbours as came to relieve them. He was however himself apparently fresh and untireable—the pain seemed to give him a charmed strength, which no sleeplessness or exertion could wear out, and he directed the movements of the attendants, and the application of the fomentations with the same anxious impatience which he evinced in the first hour of his attack. He had as yet never moved a pin's point out of the position in which he lay when I first saw him; although the bed was soaked in wet under him with the fomentations. He had derived no benefit whatsoever from the opium and calomel, although he had continued them regularly. I now ordered him a mixture composed of one ounce of volatile tincture of guaiacum, half an ounce of wine of colchicum, and half an ounce of laudanum—two teaspoonsfuls to be taken every hour in water, until some relief was experienced.

On the following morning, the third of my attendance, I was exceedingly disappointed to find that he was if possible worse than I had yet seen him. He had taken all the guaiacum, colchicum, and laudanum, during the night without the slightest relief, and the first exclamations I heard from him on entering the room were "Good God Almighty! cut the legs off of me—for Heaven's sake cut the legs off of me—do dear doctor—cut them off of me—give me ease some way, where's the hot water—Mary—Jim—what are ye all doing—oh, Lord ye don't care what I suffer—the flannels again—there—there—that way—the sole of my foot Jim—the sole man—the shin, now the shin—there's a good fellow—oh God can't ye get a hatchet at once and chop 'em off of me?" I was much more alarmed however by the state of

his bowels than by the pain. They were again confined by the opiates—the abdomen was immensely distended and tympanitic—the stomach sick, and sometimes retching, probably from the colchicum, and his countenance sunk and dejected. A large dose of castor oil and spirit of turpentine was immediately thrown up into the bowels with a considerable quantity of water by means of an enema pump, and copious evacuations soon took place with perfect relief. The pulse, respiration, and state of the skin still continued natural.

Many will probably wonder I had not as yet resorted to general blood-letting; I could not however persuade myself the affection was inflammatory, and even viewing it as such, especially as it was probably of a rheumatic character, I had the most perfect confidence that it would give way to sufficiently large doses of opium; the event proved that it would not, and from a practical inference rather than as a matter of principle, I had at last recourse to the lancet. Twenty ounces of blood were taken from the arm, and sixteen good leeches were applied to the nearest part of the loins we could reach. The only relief obtained since the commencement was experienced in half an hour after the bleeding. The poor man admitted he was easier, and for the first time for three or four days permitted the attendants to relax in their application of the flannels. A very large blister was now with some difficulty shifted under the back, and an anodyne injection with half a grain of acetate of morphia was administered for the night. He passed the only quiet one he had had since the complaint began. The pain however still continued to recur in the same parts of the leg, and grew somewhat worse in the course of the day. The calomel and opium pill was therefore again resorted to although his mouth was a little sore. His bowels were well freed in the evening by a purgative enema. At night the anodyne enema was repeated, and again procured him a tolerable night. The pain however came on in the morning again, though not at all with its former violence.

I now gave him moderate doses of sulphate of quinine, sometimes with an opiate throughout the day, and at night an injection with three fourths of a grain of acetate of morphia, and seven grains of the quinine—the fomentations were still occasionally resorted to, and the bowels were kept regular, under

which treatment the pain gradually subsided, but he walked lame, and with the assistance of a stick for some weeks after.

Although the employment of opium alone is so objectionable in inflammatory affections of the brain and spinal cord, and even in high degrees of irritation existing in those organs, there are certain combinations of it with other powerful remedies, which accomplish manifest good and do not appear to be followed by any of the ill effects consequent upon the administration of the simple drug. Combined with emetic tartar in sufficient doses it seems to possess an extraordinary power of allaying nervous irritation, quieting increased action in the capillary vessels and inducing sound and refreshing sleep. In very many cases of puerperal mania, of delirum tremens, and in the delirium which arises in the advanced stages of fever, its effects are wonderful. Medical science is much indebted to Professor Graves of Dublin, for having directed the attention of the profession to this valuable combination, and pointed out so accurately its proper application.*

With respect to the other usual combinations of opium, those for instance with ipecacuanha, and with camphor, both manifestly evince a power of diminishing the congestive or stupifying effects of the opium, and the latter as obviously tends to lessen or prevent the nausea so readily occasioned by the one, and the excitement produced by the other.

Of all the wonderful influences however exerted by opium, that by which it sustains the powers of life when sinking from hœmorrhage, and arrests the flow of blood, is the most extraordinary. When after severe uterine hœmorrhage, the countenance is sunk, the eye hollow and glassy, the lips blanched, the skin cold, and the whole person corpse like, when the pulse is almost gone at the wrist, when the beat even of the heart is scarcely perceptible, and stimulants, even brandy or rectified spirits are either vomited or uninfluential, there remains yet one remedy capable of restoring the patient to life, and that is opium. I believe its power of saving life in these circumstances depends principally on its specific property of producing congestion in the

* Dr. Graves directs four grains of Emetic Tartar, and two drachms of Laudanum to be mixed with half a pint of camphor mixture; two table spoonfuls of which are to be given for the first dose, and one every half hour afterwards until the delirium abates or some sign of drowsiness appears.

brain. That amount of congestion by which it occasions apoplexy when given in large doses to persons in health, seems only sufficient to sustain the natural and necessary tension of the cerebral vessels in those who are dying of hemorrhage. Persons die in cases of hemorrhage, not so much from mere debility of the heart's action, as from the loss of nervous power in the brain consequent to it, and hence a fainting from which they are never awakened. The opium in such cases not only stimulates the hearts action, but restores a sufficient degree of tension in the vessels of the brain to prevent faintness, and by the judicious repetition of the remedy, life is preserved on the very borders of death. There are no instances in which opium can be given so freely, or so fearlessly as in these. When the danger is imminent five grains may be given at the first dose, and two or three every hour or half hour afterwards, until the pulse becomes distinct, the breathing easier, and the tossing or flinging about in the bed is allayed. It is hardly necessary to observe, that in such cases in conjunction with the use of opium, the administration of warm wine and brandy, (however inefficient alone) and the application of heat to the extremities are highly useful, if not absolutely essential.

It is a singular fact, that in similar states of debility occasioned by acute or chronic disease, or by other causes than hemorrhage opium is utterly inefficient in sustaining life, even where the disease has already gone by. I have seen patients after having struggled through some acute disease, dying of debility with all the symptoms and appearances of persons dying of hemorrhage, the same feeble pulse, corpse-like look, cold skin, and the tossing and flinging about in the bed, so familiar to the physician in cases where alarming quantities of blood have been lost, yet opium which would have acted like magic in the latter, proved wholly unavailing in those I have described.

I have permitted these observations to run to a greater length than I had intended, or than their originality merited. Presenting as they do, the result of many years experience in the application of a medicine of surpassing power, and in most extensive use, I trust however, they will be found not wholly uninteresting to the profession.

PROBLEM XI.

BY WILLIAM GRIFFIN, M. D.

WHAT PRINCIPLES SHOULD REGULATE THE TREATMENT OF HEMOPTYSIS?

The foregoing question is not proposed with any view of entering into a discussion, on the modifications of practice, which the various organic lesions or conditions of the lungs or heart, giving rise to hemoptysis, may occasionally demand; but simply to direct attention to some principles of treatment, common to all hemorrhages, and which while they enable us successfully to controul such as are merely functional, and admit of cure, are not wholly useless even in those deplorable ones, whose removal is beyond the reach of medical art.

I am the more disposed to offer some observations on this subject, although so much better understood than it was a few years since, from my conviction, that the depleting and strictly antiphlogistic plan of treatment so prevalent in the days of Cullen, is still very unflinchingly and indiscriminately pursued by a great number of respectable practitioners. In my earlier medical experience, no fact surprised or depressed me more than the little success sometimes attending it, and when I began subsequently to contrast the many failures I had witnessed, with the many natural recoveries I had seen occur in cases which had been wholly neglected, my confidence was shaken in a still greater degree. There is perhaps no medical experience more valuable to the observant physician than the experience of neglected cases. He learns from them the natural progress of disease—he has an opportunity of estimating the extent of the retrieving powers of nature, and may form some inference as to the probable termination of the case, when neither disturbed nor influenced by reme-

dial measures. For my own part I could not listen to the statements of their cases given by patients applying for admission into hospitals or for relief at the dispensary, without feeling that until I heard them, I was wholly incompetent to pronounce on the value of the medical treatment which I was in the daily habit of employing. It was not an unconmon occurrence for a patient to inform me that he had thrown up a half pint, a pint, or a quart of blood by coughing some nine or twelve months before, that he never made any change in his diet of potatoes or meal on the subsequent days, that his appetite was rather encreased than otherwise, and that he had no return of the spitting or coughing of blood—or but a very inconsiderable one up to the moment at which he detailed the circumstances to me. He was then probably suffering under tubercular phthisis, or with simple dyspnœa, faint respiratory murmur, and dullness of sound on percussion in one of his lungs—but he was *living*, and free from hemoptysis, which was more than I could have anticipated in many of the cases adverted to within two months of the first occurrence of the hemorrhage, had they been submitted to the pure and rigid antiphlogistic treatment. I was informed too of a recovery in one instance, from periodical or occasional spitting of blood to the amount of from four to ten ounces at a time, by the patient's having impatiently broken through his usual abstemiousness, and drank freely for an evening of warm punch. It acted abundantly on his kidneys, and he had no return of the homorrhage afterwards. I knew another case, in which the patient changed from a milk diet, to which he had long been confined by his physician, to a free use of meat. From the moment he did so, all his phthisical, as well as his hemoptysical symptoms disappeared and from a very emaciated condition, he soon attained a muscularity and bulkiness of frame, which precluded all suspicion of his having been threatened with consumption.

These instances are not offered with any view of patronizing either neglect or free living in such dangerous cases, or of throwing disparagement on principles of cure, advocated by the most celebrated medical men. I merely desire to restrict the extent to which those principles should be pushed, and to point out the necessity of keeping them subservient to laws of more general application. Perhaps my object may be better understood by relating a melancholy case which first impresssd me with a

deep sense of the importance of attaining correct views on this subject.

A youth of seventeen years of age, was attacked with spitting of blood, occurring to an amount of three or four ounces at intervals of six or eight days. He was a well formed muscular young man, and had never shown any symptoms of phthisis, or of any other serious disease, previous to his late seizure. Happening subsequently to get an attack when at some distance from home, he became alarmed, and getting into a carriage drove ten or twelve miles to his own neighbourhood. I met him the moment he arrived, and seeing that he was spitting blood profusely had him instantly placed in bed, with few bed clothes, his head and shoulders elevated, and the windows thrown up. He had salt and cold water to drink, being the readiest astringment I could procure. There was however no abatement of the hœmorrhage, and if I except cases in which the patient died instantaneously, I never saw such frightful quantities of blood thrown up in so short a time. I had him therefore taken out of bed again, placed in a chair, and a can of cold water poured over his chest, upon which, the violence of the discharge almost immediately diminished, and finally, under the influence of other remedies, it ceased altogether.

A very experienced physician met me afterwards in consultation, and in conjunction with the ordinary astringents employed in such cases, we directed a cold solution of the supertartrate of potass to be given in very small quantities as common drink, and forbade all nourishment. The hemorrhage returned every second or third night, to an amount of from four to eight ounces at each turn. Every description of astringent was employed without avail, and sometimes a bleeding from the arm to the amount of six ounces was resorted to on the evening the return of the attack was anticipated, with no other effect than that of deferring it to the ensuing night. The case ran on for about three weeks, but what I wish particularly to direct attention to, is, that during the whole period the physician who attended in consultation with me, would allow of no nourishment, and no drink whatsoever beyond the solution of cream of tartar. The greater the hemorrhage, the more alarm he expressed at the danger of giving the least sustenance, believing that if any possibility existed of suppressing the discharge, it could be effected only by the most absolute rest

in the horizontal posture, total abstinence from food, and the administration of acid drinks in spoonfuls from time to time.

Towards the close of the case under this management I believe no blood had been thrown up for some days, and though the pulse was exceedingly rapid and compressible, hopes of possible recovery began to be entertained. One morning however, when the poor fellow raised his head to take a drink, I observed the cup drop from his hand. On looking up, I perceived an appearance of fainting—a death-like paleness overspread his countenance, his head fell back on the pillow, and in a moment he was no more !

I was too young in the profession at the time to have had any well founded opinions as to the propriety or impropriety of the practice adopted, nor can I now with all my subsequent experience venture to say, any treatment whatsoever would have given a result materially different in so alarming a case. But of one thing I am convinced, that in all such diseases the patient should be allowed to die of the hemorrhage, rather than of the debility following the interdiction of all food. Neglected cases daily prove to us, that even very alarming hemorrhages may not prove immediately fatal although no abstinence be practised, while no one can doubt the certainty of speedy death if blood is daily bursting from the lungs, while all possible supplies in the way of food are cut off from the circulation.

Setting aside however, altogether, those extreme cases in which no possible treatment can succeed, I am still strongly impressed with the belief, that the principles upon which this rigid abstinence is imposed, is in almost every instance of protracted hemorrhage an erroneous one.

Adverting to the old and practical distinctions of hemorrhage into active and passive, it will I believe be readily admitted, that although the former character may apply to the majority of cases at their commencement, signs of passive disorder are very soon apparent, and require a treatment directly the reverse of that demanded at the onset. This is no case so obvious, as in simple uterine hemorrhage arising from plethora, or some accidental determination of blood. Depletion, and the antiphlogistic treatment, however they may have controuled or abated the attack at the commencement, if it has not wholly given way are soon found to be utterly unavailing. Whatever the origin of the dis-

charge may have been, we find that its continuance depends on a want of fibrine in the blood, and of tone and contractility in the capillaries; and the hemorrhage that at the onset would have been aggravated by the slightest stimulus, is perhaps at last, and at once suppressed by the exhibition of port wine or claret. I have seen many cases of simple menorrhagia cured by stimulants and chalybeates after the less exciting astringents had totally failed; and I well remember one remarkable instance in which the complaint, after resisting every possible variety of treatment, yielded immediately on the application of a blister to the sacrum. In strumous habits especially, in which the capillary vessels soon lose their tone, hemorrhage, whether from the lungs or uterus, when it has assumed a passive character, is greatly relieved by the application of a blister to the neighbouring surface, provided it be not so large as to excite any general febrile action.

The advantage of preserving the contractile power of muscles in cases of hemorrhage is in no instance so striking as in uterine hemorrhage after an *exhausting* labour. If the old wives' custom of administering a cordial, such as gruel well warmed with nutmeg or caudle, immediately after delivery, is adopted, the uterus contracts actively and the loss of blood is trifling, but if, *when there is excessive exhaustion or debility*, the apprehension of hemorrhage prevents the exhibition of any stimulant, an alarming flooding will in all probability take place. The same deficiency of tone or contractile power is experienced in the most minute capillary vessels, as in the fibres of the uterus itself, and is remedied by the same means, the exhibition of some stimulant or supporting treatment. This is obvious in the violent hemorrhage we sometimes meet with from the unimpregnated uterus, where the suppression of the discharge depends upon contraction of the capillary vessels and not upon contraction of the uterine fibres. Cold externally applied, by some sympathetic influence effects this contraction to a certain extent; but when cold fails, and the flooding goes on, and the lips are bloodless, and the extremities chilled, burnt brandy, claret, and other stimulants very often accomplish an arrest of the danger. I have seen an analogous instance in a case of bronchial membrane, in which, after all the usual antiphlogistic and astringent remedies failed, the patient was cured by the administration of muriated tincture of iron and ordinary ale.

I am chiefly anxious to insist upon the fact, that in all protracted and recurring hemorrhages, which are capable of being controuled by treatment, the time arrives when the flow of blood may continue or recur from the mere want of tone and contractile power in the capillaries occasioned by pure debility, and that if the same rigid abstinence from nutriment, which was judiciously imposed at the commencement, be persevered in, the hemorrhage will be kept up. There may be no doubt in many, such a disposition to a recurrence of the original hemorrhagic action, or the irritation which occasioned it, as soon as the heart and arteries begin again to recover their proper tone; but if there be any truth in the principle laid down, this circumstance cannot at all affect it, referring as it obviously does, rather to the extent to which nutriment may be given, than to the propriety or impropriety of giving it. I can well imagine there may be some difficulty in convincing practitioners who have been long wedded to the actual starvation system, that nutriment can be administered at all with safety while bursts of hemorrage are recurring every third or fourth day. Independent of their preconceived and prejudiced notions on the subject, it frequently happens when they have ventured on giving an occasional spoonful of arrowroot or of chicken broth, that slight febrile action, and after a few hours, perhaps fresh spitting of blood has come on which is naturally attributed to the exhibition of the nutriment. To form a fair inference on the subject however, it should be considered, that at the commencement of such cases, recurrences of hemorrage often take place, perhaps every third or fourth day whatever treatment is adopted; that the excitement occasioned by the nutriment is often rather the effect of a morbid irritability of the vascular system, arising from prolonged starvation than of its direct stimulant power; that this irritability, like the hemorrhagic action of the pulse following extreme losses of blood, will diminish in proportion as we can gradually restore strength to the system by the administration of the mildest nutriment; and that it should more properly suggest the necessity of giving sedatives to allay it, than of recurring to a system of prolonged starvation, which in the highest health no constitution could resist, and under which in disease no reparative process could possibly be accomplished.

There are indeed dangers both immediate and remote, attendant on extreme depletion and abstinence in hemoptysis, which are not

always held sufficiently in view by the medical practitioner. One of the immediate consequences to be apprehended from an attack, is infiltration of the bronchi and lungs with the effused blood, and capillary congestion of them, which by creating irritation, and interfering with the respiratory function, tend to produce a recurrence of the hemorrhage. Now a period of prostration of the strength arrives in all protracted hæmorrhages, and is obvious in the very commencement in some, when this consequence instead of being probable only, will be made certain by further depletion or abstinence. The vital tone of the capillaries will be thereby diminished, and in proportion the tendency to infiltration and congestion will be increased. It will even sometimes happen, as Dr. Robert Law justly remarks,* that the powers of life may be reduced so low as to oblige us to administer stimulants, and although there is not a nicer point in the practice of medicine than determining the propriety or amount of their administration, that circumstance cannot make the occasional advantage of resorting to them less certain.

But there is another danger attendant on frequent depletion and protracted low regimen, which though remote is not the less alarming—the danger of inducing Phthisis. Of what avail is it that we have succeeded in finally and completely arresting the hemorrhage by this practice if the result be that our patient run into consumption and die? It is well known to medical men, that in most cases hemoptysis occurs in constitutions predisposed to tubercular Phthisis, and it is equally well known, *that in such constitutions tubercles are readily generated by any deterioration of the vital power.* A quart of blood taken from a consumptively disposed female in perfect health will often lay the foundation for Phthisis. She has been lowered beyond the reach of the retrieving power, there is weakness and irritability of the nervous and vascular systems—insidious disease sets in—she becomes gradually hectic and dies. Is not this the history of numberless cases of consumption which follow severe floodings during a lying in? or a deteriorated condition of the system from starvation, or from protracted disease? and does it not then become us, as conscientious and provident physicians, to use our weapons with

* Cyclopedia of Practical Medicine. Article, Hemoptysis.

foresight, and starve with discretion, even in combating so alarming a complaint as hemoptysis.

There is the less defence for the practice I am contending against, as we are not without remedial agents which are little inferior to bleeding for the arrest of hemorrhage, yet leave no permanent debility. I have seen very frightful cases of hemoptysis subdued by an emetic of Ipecacuanha, and by the continued administration of it afterwards in grain or two grain doses every hour. Emetic Tartar has also been successfully employed, and when once the attack has assumed a passive character it is well known that many other remedies prove highly influential.

I may mention in conclusion, that my object in this discussion has not been to enter into the details of treatment proper to hemoptysis, which must vary in almost every case, nor to question the general propriety of depletion and abstinence; but to point out that the principle upon which that practice rests has its limits, and that in almost all cases when the powers of life become reduced to a certain degree, a new principle comes into view, and as a consequence a different treatment.

PROBLEM XII.

BY W. GRIFFIN, M. D.

HOW SHOULD ACUTE RHEUMATISM BE TREATED?

Greater improvements have been achieved in the treatment of acute rheumatism within the last few years, than in that of any other disease attended with equal suffering or danger; and the above question is not proposed, because of any deficiency of influential remedies, but rather on account of the doubts which arise as to their comparative merits—a matter of no small moment, when one considers the frequent fatality of the disease, notwithstanding the boasted efficiency of medical treatment. It will appear too upon inquiry, that one or two plans of management, to which I shall by and by more particularly call the attention of the reader, are far and away preferable to others, that none are applicable to all cases, and that when we shall be able to discriminate with more precision, the states of the constitution, or the disease to which each method of cure is particularly applicable, we shall have accomplished all perhaps that medical art is capable of in this distressing malady.

The late Dr. Hope in his concise, but admirable essay on this subject, has given a fair review of the several methods of cure then in repute, and contrasted them with his own. More lately, Dr. Corrigan, in a paper published in the Dublin Journal of Medical Science, has given a clear and able statement of the successful treatment of the disease by pure opium, administered in larger and more frequent doses than had hitherto been resorted to, and has shown the superiority of this remedy over the antiphlogistic treatment of Sydenham and Bouillaud. If these essays obtained their deserved influence over medical practice, it would

perhaps be superfluous to raise new discussions on the subject. But other, and inferior plans of treatment are attended with such occasional, and even general success—(no extraordinary occurrence in a disease, which, if let alone, will in perhaps a majority of cases, after much and protracted suffering, cure itself) that practitioners are still continually diverted from those of a far higher character—almost certain in their results, and absolute guarantees from danger. It may be well too, to offer a few observations as to the respective merits of the treatment proposed by the late Dr. Hope, and that by Dr. Corrigan, to which the latter physician has not adverted.

In any fair and sufficiently extensive enquiry on this subject, it will probably be found, that no one remedy or plan possesses all the perfections attributed to it, that the influence of all of them will vary with the state of the constitution, its idiosyncrasies, and the relations of the disease with other morbid conditions which may be co-existent with it, and that remedies, altogether inferior, and by no means generally applicable, will in some instances effect a recovery, where those almost universally influential may fail. Our object should obviously be, to ascertain as accurately as possible, the conditions of the system, or of the disease, which may render one remedy occasionally more appropriate than another, as well as to determine the precise value of such, as are generally applicable. To effect this, much more extensive and accurate observations are necessary than either the present records of medicine or our own experience can supply, and it can at any time be attained only, by considering as Dr. Corrigan has done, what remedy effects a cure.

1.—In the shortest space of time?

2.—With the least pain, or other symptom likely to protract or interrupt the cure?

3.—With the least probability of inflammation of a vital organ?

4.—With the least after ill effects, whether debility, salivation, or chronic disease, and with the least liability to relapse?

It is now well ascertained, that in perhaps a majority of cases rheumatism admits of a natural cure, and that if an antiphlogistic regimen be observed, such cure may be very perfect, and sometimes take place within four or five weeks, or less. Sydenham's treatment latterly, after he began to lose confidence in bloodletting, was very much what has been called "expectant," a proof

that the disease if not mischievously interfered with, will very generally terminate favourably. Dr. Zeroni of Manheim was so impressed with this fact, that he considered pericarditis and endocarditis (so often the result of acute rheumatism) as artificial complaints, produced entirely by unnecessary interference with its natural progress. If however, we examine the instances adduced in favour of such opinions, or refer to neglected cases, which every physician meets with in abundance among the poor, we find that even in the less violent attacks the complaint is usually protracted to four or five weeks—that it is for the most part attended with extreme suffering, and that it leaves a great liability to relapse. We find further, in direct variance with the inferences of Dr. Zeroni, that affections of the heart are a frequent consequence of allowing the disease to run its natural course, and that under such circumstances, it very commonly terminates in chronic rheumatism, if not in extensive organic disease and death. When it is considered, how very intractable chronic rheumatism is, and how perfectly and speedily the acute form admits of cure, such a termination must be regarded as a very deplorable result.

Holding these facts in view, we shall be able to estimate more accurately, the influence of various modes of treatment, and how far any of them may be capable of altering the natural course of the disease.

Large and repeated bleedings, as our experience of rheumatism and our confidence in other remedial measures increase, are fast falling into disrepute. However ingenious the argument which would identify rheumatic inflammation with the ordinary phlegmonous inflammation of other parts, or however successful a bold antiphlogistic treatment may occasionally prove, there are certain differences in their characters which never have been, and I believe never can be explained. Rheumatic inflammation migrates from joint to joint, and from organ to organ, and muscle to muscle, in a manner wholly unknown in simple inflammation, it shews no tendency to adhesion, suppuration, ulceration, or mortification, and it sometimes resists the most active depletion with an obstinacy which cannot but be regarded as peculiar to it. The frequent cure of acute rheumatism by the antiphlogistic treatment seems readily explicable, if we consider that in its milder forms it runs a certain course, and terminates by a natural cure—that in proportion as inflammatory fever runs high, the disease becomes

more severe, and the chance of a natural cure is diminished; and on the other hand in proportion, as inflammatory fever is reduced by active depletion, the natural tendency of the complaint to recovery is uninterfered with and perhaps promoted. Whenever the antiphlogistic treatment fails to reduce the amount of inflammatory action, or produces only a temporary reduction of it, no natural disposition for recovery arises, but in its stead, symptoms of inflammation of the heart are observable, or if not, a tedious convalescence or broken constitution is the result. Dr. Hope has given all this in his forcible and graphic essay on the subject:— "many cases" says he "were promptly and effectually cured, even annihilated at once by the antiphlogistic plan; but in many others, active bleeding was carried to the very last ounce that could be drawn, yet the enemy clung to the joints with a chronic grasp, and proceeded triumphantly in his crippling career. "I can fully bear out that lamented physician, in his statement of the ill success of the pure antiphlogistic plan in the Edinburgh Infirmary, as far as I had an opportunity of estimating it. I remember witnessing there, the case of a stout young man who was treated on this plan, and whose complaint seemed almost to disappear after each large bleeding. It nevertheless recurred every second or every third day, until the system was absolutely drained of its blood, and though the sufferer became pale as a sheet, the tongue continued loaded, and the pulse retained its rheumatic throb. The heart at length became attacked, and he died. I witnessed another case in dispensary practice, which had a similarly unfortunate termination. The patient was a young girl of sixteen. There was an obvious improvement after each bleeding, but the disease went on, and acute inflammation of the heart with high delirium supervened. One morning on calling to visit her, I missed the patient from her bed. On looking round with some alarm, I at length discovered her, standing within the folds of the curtain at the bed's feet, her figure erect and drawn up, her face wan and bloodless, her eyes dilated, and her whole appearance suggesting the idea of a spectre, rather than of a living being. She died a day or two after. In cases either totally neglected, or less actively treated, I do not remember before or since to have witnessed a fatal termination take place so rapidly as in those two instances, and when I consider this fact in connexion with Dr Alison's remark, that large and repeated bleedings in the begin-

ning of rheumatism increase the risk of metastasis," and with the ill success attendant on the "coup sur coup" practice deducible from the reports of Bouillaud, to which Dr. Corrigan has directed attention, I cannot but feel that no occasional success however perfect, or extraordinary, can any longer warrant medical practitioners, either in pursuing it too boldly, or placing exclusive dependence on it. It will indeed appear that the average period occupied in the cure by it is longer, the success more doubtful, the pain more considerable, the danger of metastasis and of relapse greater, and the convalescence more tedious on comparing it with other methods to which we shall presently advert.

The sweating and stimulant plans of treatment formerly popular are now so universally abandoned, that it is almost needless to notice them. The patients, as Dr. Hope has remarked, recovering in spite of the remedies rather than in virtue of them.

The colchicum treatment when pursued in connection with moderate depletion and an antiphlogistic regimen, yet maintains the reputation which its success in the cure of rheumatic ophthalmy would lead one to anticipate. Dr. Law states, that in his practice a cure was almost always effected in from ten days to a fortnight, which is much about the duration of the more favourable cases treated by Bouillaud, on the antiphlogistic plan. With either physician, we believe a large number required a longer time for their cure, and it was altogether less certain. We have also experienced an inconvenience in the use of colchicum, of which Dr. Hope complains—the occasional occurrence of obstinate, and even dangerous diarrhea. There can be no doubt the treatment by this powerful drug is generally successful, but it is on the whole, less certain, more painful, and sometimes attended with more serious consequences than other remedial plans.

Emetic tartar has been extensively used (in accordance with the contra-stimulant theory suggested by Rasori,) for the cure of rheumatism. Laennec, Louis, Lapelletier and others administered it with great boldness, and with great occasional success. Its heroic powers are only observable, when it is given in doses which would have been formerly regarded as poisonous, and whatever its mode of action may be, it is obviously identical with that by which calomel effects a cure in Asiatic cholera, and opium arrests almost every form of acute inflammation. Laennec gave a grain every second hour, then desisted for a few hours, then

resumed its use again. In very severe cases, he continued it all through, so as to give from 12 to 24 grains daily. Louis gave it in doses of from 1 to 2 grains, varying the intervals, so as to administer from 6 to 12 grains in the day, and both combined it with opium. I have myself given it uncomplicated with other remedies, and with much more freedom than it was employed by Laennec or Louis. The cures it sometimes effected seemed almost miraculous in their instantaneousness and perfection. I was quite amazed at the sudden relief it gave in the first case I employed those larger doses in. I was summoned to see a middle aged woman, who had been three or four weeks on her bed in acute rheumatism. The inflammation had gone on shifting from joint to joint in despite of repeated bleedings, and other depletory remedies, and at last rendered the patient wholly incapable of motion. She lay fixed and pale in the bed, unable to move a single limb, with a white tongue, a compressible but throbbing pulse, and distressing palpitations. On taking a grain of tartar emetic every hour, for 12 or 14 hours, all her pains subsided, and she could move and turn herself in the bed without any difficulty; the palpitations also subsided. I did not see the woman after, but heard from the apothecary who was carrying my directions into effect, that her recovery was rapid and perfect. The following cases taken from the books of the county of Limerick infirmary show not only the speed with which a cure may be sometimes effected by this powerful drug, but the freedom with which it may be administered.

Case I.—Mary Molony admitted Sept. 23. Suffering with acute rheumatism of three weeks continuance. The lumbar region, left hip, knee and shoulder are the parts principally affected, and the usual rheumatic fever and pain are present. Habeat Antimonii Tart gr. i. omni horâ.

Sept. 25.—First and second doses thrown up, the subsequent ones retained. Has now taken one grain every hour since the 23rd, except while she slept. Is much better—*pains almost gone.*

Sept. 27.—All pains gone, but complains of a bitter taste and nausea. The emetic tartar was now discontinued, for the purpose of giving a purgative mixture, but was subsequently resumed. On the 30th. she was discharged cured.

Case II.— Mary Donoghue aged 33 years. Married 17 years, admitted Feb. 18th, in acute rheumatism of 14 days duration— suffers much pain—elbows, shoulders, neck, loins, knees, and wrists affected. Tongue whitish, no sleep, pulse 100, thirst moderate, appetite pretty good, bowels confined.

Habeat: Antim. Tart. gr. i. secundâ quâque horâ.

Feb 21.—Got speedy relief which has continued up to the present moment. Pains all gone, pulse 90, feels better in every respect. The solution was subsequently directed to be given every third hour and gradually left off, and full bread diet allowed; she had no recurrence of the symptoms, and left the house on the 28th.

Cases such as these in which the cure was effected more rapidly than I have seen it by any remedy in use, would lead one to imagine the true specific for this painful disease had been at length discovered: but when the practitioner tests the remedy more extensively he finds, not only that it is often attended with inconvenience, and if injudiciously pushed, with hazard, but that its failures are considerable in point of number. With Lepelletier they amounted to half those in which he employed it, and the success was little better, even when accompanied by bleedings. In the Limerick Infirmary, I believe the failures were more than two thirds. Louis and Laennec had greater success, perhaps because they usually combined it with opium. The first and second doses almost always vomited, and when the remedy was borne well after, a cure frequently followed. The occurrence of purging did not always arrest the improvement of the patients and was often easily suppressed, but sometimes it held on, and when there was great debility awakened apprehensions of danger. The cases were however many, in which tolerance of the remedy never took place at all, and where consequently it could not be persevered in. On the whole, it appears evident, that while emetic tartar administered in large doses occasionally displays a singular and extraordinary controul over acute rheumatism, we are yet at a loss to determine in what cases it is more particularly successful, or how its influence can be more extensively applied.

The next and most important remedy to which I shall advert is opium, whose virtues Dr. Corrigan has so satisfactorily demonstrated, when given in doses of one or two grains every second or third hour, or of ten or twelve grains, or more in the twenty four

hours. The extreme suffering attendant on rheumatism has at all times driven physicians to employ it as an adjuvant in the cure, and under the sweating treatment formerly so much in vogue, its administration was carried to a very considerable amount. But as it was never given as the sole and principal remedy, its failure was little remarkable. The qualified success of its employment in the hands of so acute an observer as Dr. Hope could only have arisen from his not administering it in contra-stimulant doses When he used it alone, or in combination with purging, and perhaps one moderate bleeding at the commencement of the disease he states that he found the cure less expeditious and certain than when given on his own plan along with calomel. There is great credit due to Dr. Corrigan, for having been the first to apply opium to the cure of acute rheumatism in the large and repeated doses which have been found so successful in subduing acute abdominal inflammation, and in pointing out the total uselessness of the remedy, when given in the minute doses in which it has been ordinariily employed. "The most important rule to be remembered" says Dr. Corrigan, "in employing opium for the cure of acute rheumatism is, that *full and sufficient doses* shall be exhibited. I have heard of opiate treatment having disappointed those who have tried it. On enquiry, I have learned that in those cases it has been given only to the extent of a grain every fourth or every sixth hour. This is not the treatment of rheumatism by opium ; it is making the patient worse than before ; it is inflicting on the patient, the mischief arising from the stimulant effects of the drug, and withholding from him all the benefits of its sedative influence. The opium should always be increased in dose, both as to frequency and quantity, until the patient feels decided relief; and should be then kept up at that dose until the complaint is steadily declining. The first indication that tells the practitioner he has reached the proper dose, is the statement of the patient, who in reply to an inquiry, as to how he has passed the night, probably says that he has not slept, but that he is free from pain and feels comfortable. This effect having been attained the opium may then be continued in repetitions of the same dose as to frequency and quantity."

Dr. Corrigan states the average cure under this treatment to be nine days. The remedy was not followed as might be expected by constipation, on the contrary, purging sometimes took place

under its use; but even where the bowels were not moved for two days, he saw no disadvantage arise from it. There was little suffering during the treatment, little debility as a consequence, and only one attack of the heart in all his cases.

It is obvious that no plan of treatment previously referred to, can approach this in importance, but before we offer any more particular comments on it, we shall give an outline of Dr. Hope's plan, (the only one which can at all compete with it) in his own words.

"After a full venesection, or even two in the robust, but without bleeding in the feeble and delicate, I give every night gr. v. to x. of calomel, with from a grain and a half to two grains of opium, according to the age and severity of the case, and every morning a full purgative, to act four or five times at least. In addition, I generally give the following draught three times a day, as it has appeared to me to expedite the cure—partly, perhaps, by the additional opiate, and partly by the sedative effect of the colchicum.

R. Vini. Colch. m. x. ad xx; Pulv. Doveri, gr. v. Mist. Salin. drachmas decem, Syrupi drachmam. M.

"When the pain and swelling are greatly abated, if not almost gone, which often happens within two days, and almost always within four, I omit the calomel; and if the gums become in the slightest degree tender, I omit it even earlier. The opium I continue to the extent of gr. i. or iss at bed-time; and in severe cases I add a grain at noon; for without an anodyne the pains are apt to recur. I also continue the colchicum draught, and the senna purgative as before."

"No local treatment is necessary beyond warm or cold applications, according as the patient finds either agreeable."

"If the patient is not well in a week, I consider it a case of exception; and the exceptions are generally in those who are subject to rhumatism, and who therefore have it in a more obstinate chronic form."

"The advantages of this plan are, first,—that a patient is generally sound, well, and fit for work in a week or ten days after the pains have ceased; secondly, that the gums are rarely affected, especially, if you previously ascertain that the patient has not a morbid susceptibility of mercury; thirdly, that it is rare to see inflammation of the heart, if the treatment is early begun, (I think

that one case in a dozen would be the maximum in my practice); fourthly,—if the slightest symptom of endo, or peri-carditis *does* supervene, a few extra doses of calomel and opium administered every four or six hours will generally affect the constitution in twenty or thirty hours, which, with two or three cuppings or leechings on the region of the heart, almost always places the patient in a state of safety. I never lost a patient by rheumatic pericarditis since I employed this plan; and I have been told by other hospital practitioners, that they have been equally successful in the use of calomel and opium."

If we now compare these two plans of treatment, and enquire which effects a cure in the shortest space of time, we find the difference between them is very slight. Dr. Corrigan's cases were cured on an average in nine days. Dr. Hope looked upon the case as an exception, if not well in a week. If it be asked, which is the least painful, the difference is perhaps equally trifling. Dr. Hope though not giving the opium so frequently, gave a larger dose at night, and at any rate, as far as I can form a correct estimate from my own experience, I can state, that the relief from pain is generally as speedy, and as perfect as if it was exhibited hourly. The proof of this is obvious in the fact, that patients seldom complain of the severe purging from the calomel and senna which they would assuredly do if the pain was not cotemporaneously alleviated It sometimes indeed happens in very severe cases of synovial rhumatism, especially of the hip joint, that the patient cannot allow of the slightest movement in the bed, and screams even if the bed clothes be touched. But these are exceptions, and can be treated on no one plan of cure solely. They must be met by bleeding, leeching, opium, calomel and opium, and blistering, in short, by all the more powerful agents for the arrest of acute local inflammation, as the supervention or obstinacy of particular symptoms may suggest. I recollect a case of this nature, in which I had sometimes to adopt Dr. Hope's plan—sometimes Dr. Corrigan's during an agonizing and protracted attack, and where neither would have succeeded, if their effect had not been assisted by the application of an immense number of leeches, and by blistering. The opium given hourly, for a length of time that made it exceedingly difficult to break the patient of its use afterwards, was always productive of relief, but this was almost all it seemed to effect—there appeared to be no progress made towards

recovery, and whenever it was omitted for a time, there was a recurrence of the pain. I have found too an inconvenience from the opium treatment which Dr. Corrigan notices as possible, but as not having occurred in his cases—the occurrence of stupor or rather heaviness, and head-ache, and of constipation; and though I have frequently seen the bowels regular during the hourly exhibition of opium, I cannot say I have ever seen its use accompanied or followed by diarrhea. I fear several cases will be found in practice, in which constipation and affection of head will compel the physician to resort to Dr. Hope's plan, which includes active purging. On the other hand, in treating the complaint by the latter method, there are perhaps an equal number in which the supervention of sore mouth will suggest the advantage of completing the cure by opium, or if the pain is nearly subdued, by quinine or hydriodate of potass. It must nevertheless be held in remembrance *that salivation can never occur in these cases any more than in abdominal inflammation or in cholera, unless the use of calomel be persevered in after the symptoms have completely given way, and a cure is in part effected.* So long as acute disease lasts, mercury will not salivate, but a single dose given after the disease has given way may do so, and the difficulty of avoiding it generally arises from the influence of our own apprehension, which tempts us to continue the remedy beyond the absolute necessity for fear of a relapse. I believe by a little caution salivation can always be avoided in the treatment of acute rheumatism, as it may in any other acute inflammation.

With respect to the probability of the heart or any internal organ becoming affected, it appears to be very low indeed. Dr. Corrigan had but one case of affection of the heart among all those he treated, and Dr. Hope though meeting symptoms of pericarditis in many cases, never lost one under the calomel and opium management. Indeed, should such symptoms supervene, whatever treatment might have been previously pursued, there are few practitioners of the present time who would not instantly resort to calomel and opium in large and repeated doses, nor, regarding the rapid mischief which takes place in such cases, and its fatal tendency, could any one feel justified in placing the least dependance upon other remedies.

If we proceed to enquire which of these methods of treatment leaves the least after ill effects, and the least liability to relapse,

again referring to our own experience, we should say, that while they are immeasureably beyond all others in these respects they differ very little as compared with one another. Dr. Corrigan in speaking of the slight impairment of strength under the opium treatment, says, the patient cured by opium has neither bleeding, blistering, nor mercury to recover from in his convalescence. If this reference to mercury applies to the production of salivation, Dr. Corrigan's objection to the treatment is very fairly urged ; but if to the exhibition of mercury on Dr. Hope's plan, it does not at all apply—as that plan occasions little or no debility—less if possible than the opium. In fact, a temporary depression sometimes does follow the exhibition of opium as an after effect, and where its use has been long persevered in, as may be sometimes necessary in protracted cases of synovial rhumatism, the debility which succeeds the suspension of the medicine often lasts for days. If calomel be given so as to excite profusely the action of the salivary glands, it occasions a far greater and more protracted debility, but if it be given in large doses, and yet not pushed to the extent of salivating, (that is beyond the necessities of the complaint) little or no debility follows. Dr. Hope states as one of the advantages of his plan " that a patient is generally sound, well, and fit for work in a week or ten days after his pains have ceased." In short, it would appear that neither the opium plan of treatment nor that by calomel and opium are necessarily followed by any serious debility, and consequently do not leave after them that disposition to relapse, which is one of the worst consequences of almost every other plan, of which we have had any experience.

In reference to this subject, it may be worth while to observe, when either from natural delicacy of habit, or other circumstances the strength of the constitution seems much impaired and the disease is running into a chronic state, or when the attendant fever begins to assume a hectic character, the administration of sulphate of quinine, or of hydriodate of potass in efficient doses is often highly serviceable, far more so indeed than they have proved to be at the onset of the disorder in which they were formerly in such great requisition.

In concluding this somewhat cursory review of the several plans of cure which have been proposed for acute rheumatism, I must again advert to the danger under any treatment of the heart or pleura becoming affected, and the vigilance required on the

part of the practitioner to forestall, or meet such an alarming occurrence. All the instances of death, from acute rheumatism, and three fourths of the cases of disease of the heart by which after life is made utterly miserable, are owing to the circumstance of the physician having unfortunately overlooked, or inefficiently treated their early progress. They are often insidious in their commencement, so much so indeed, that they have sometimes gained considerable ground before even an experienced observer has detected them. But with an inexperienced or inapprehensive practitioner, a fatal amount of mischief is too frequently accomplished before he can distinguish a single token of danger.

I would most strongly recommend to every young physician the attentive study of the incomparable papers of Dr. Latham on these affections.* I do not believe in the records of medicine, any medical essays can be found, written with more force or perspicuity or in a more true and philosophical spirit, or containing so many interesting facts and reasonings in so limited a space.

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* Medical Gazette, vol. iii. p. 456, Dr. Latham's lectures on diseases of the heart

OBSERVATIONS ON THE APPLICATION OF MATHEMATICS TO THE SCIENCE OF MEDICINE.

BY DANIEL GRIFFIN, M.D., AND WILLIAM GRIFFIN, M.D.*

It must be confessed, that the science of medicine, whether we consider the length of time it has existed in the world, the high position it has a right to claim in relation to the well-being of mankind, or to the rigorous and searching spirit of the present age, stands most undeservedly low in the scale of knowledge. Many subjects have been cultivated with infinitely more ardour that were of infinitely less importance, and even those persons who have devoted themselves with the greatest assiduity to its improvement, have been extremely slow to avail themselves of those methods of investigation, by the use of which other sciences, many of them but of yesterday's growth, have sprung up and pushed onward, and left it at a hopeless distance behind.

If we look to those sciences that we now admire for the stupendous truths they reveal to us, or the brilliancy and beauty of the discoveries they include, we find that they too, like medicine, have had their days of darkness, uncertainty, and error. Astronomy,, the purest, the brightest of them, was mixed up and defiled with the vile superstition and cheating spirit of astrology. According to it, if man was born under a particular aspect of the heavens he might contend in vain against his doom, if an evil one. It told him nothing that could be useful, and little that could interest him, save that in such a case, whatever might be his conduct or character, all the "skyey influences" were leagued against him. Modern astronomy has withdrawn itself from these base trammels, and speaks of facts which are demostrable, and, though not less wonderful, in every respect practical

* The first seventeen pages of this paper are by Dr. D. Griffin—the remainder by Dr. W. Griffin.

and useful. It represents the globe he inhabits as but a speck amid the vast machinery of which it forms so inconsiderable a part, surrounded on every side by a gulf of inconceivable and frightful depth, yet taking its course through it with a regularity and steadiness so astonishing, that the mariner standing on the unstable deck of a ship, and observing the positions of the heavenly bodies with an instrument the size of his hand, can, with the assistance of a page or two of numbers from the nautical almanack, not only determine its position in space, but can even determine the point he occupies on its surface, in latitude within one mile, and in longitude within twenty. Thus, while astronomy, in the order and beauty of the mechanism of the heavens, furnishes the strongest evidence of a designing and benevolent mind, a bright contrast to the predestinating doctrines of astrology, it, in one sense, fulfils the old astrological notion of a powerful influence over man's welfare, though not a supernatural one ; for upon a knowledge of the positions and hourly changes of the heavenly bodies, are every day staked the wealth of nations, and the lives of thousands, and all this is the effect of number.

Again, turn to chemistry ; who has not heard of the wild dreams of the alchemists, their search for the philosophers stone the precious metals, and the elixir of life; their daily and nightly labours ; their wasted health, poverty, and rain, and their despairing and pathetic expressions on misspent time ; yet from such beginnings as these did chemistry arise, and by following that course which the Baconian philosophy had left her a brilliant example of in the labours of Newton, raised herself to such a position as to be denominated the queen of the modern sciences. The alchemists sought for gold ; chemistry in the acts of life has brought more treasures to mankind a thousand fold than if they had discovered it: the alchemists looked for the elixir of life ; chemistry has made more presents to the healing art than the latter well knows what to do with ; and if it must be confessed that many specifics for diseased action have not yet been found among them, this is rather, perhaps, to be attributed to the fact, that the medical art has not followed the same strict methods in their application that chemistry has in their discovery.

The same thing may be observed of the science of optics, and if this science has not yet excited the mind of man by vain delu-

sions in its early career, it is, perhaps, more remarkable than any of the rest for the brilliant and extraordinary results that have followed its investigations in later times; results which, in giving him the telescope and the microscope, have in a manner endowed him with new senses; have, by the one, as it were, lifted him many millions of miles into space, and enabled him to examine the bodies there revolving, and thus given him a kind of omnipresence; and by the other opened up a world to his view which, though contained within a smaller compass, offers a store of knowledge that will leave his industry unexhausted for centuries. These, too, are but a portion of its rich gifts; every day new discoveries are arising in it, all of them of the highest beauty and interest, and many of them of vast practical utility.

We might add to these examples by turning to other sciences, but it is unnecessary. Wherever we turn it is melancholy to observe the contrast that presents itself between the systematic and elegant arrangement of knowledge in these branches—the consequent precision with which this knowledge is applied to the questions that arise in them, and the irregularity, uncertainty, and confusion, that for the most part characterize the facts, the reasonings, and the conclusions of medical science. Take any one disease, such as fever, for example, what medical man is there, how great soever his reputation or experience, who will be able to tell what proportion of cases out of 10,000 or 100,000, are affected with the symptoms of involuntary discharges, subsultus-tendinum, hiccup, difficulty of swallowing, or any of the different combinations of these symptoms? and what proportion of these recover or die, or what proportion of cases out of the same number are seized with head affections, chest affections, or abdominal affections? and what is the mortality induced by these complications respectively? There is not so much as one man in these kingdoms who will venture to give even an approximate solution to such questions; yet the answers to them would perhaps save him a world of wasting anxiety as to the probable termination of cases, in the event of which every feeling of his heart is set. Again, take the question of the conditions proper for the exhibition of stimulants in fever. Some five and twenty years ago wine was given freely in fever; within the last ten it is stated, "that as many ounces are not now given as were then given pints;"

and more lately again we see Dr. William Stokes, of Dublin, exhibiting it freely, and, apparently, with much advantage, in particular circumstances. The opinions as to the conditions proper for its exhibition are as varying, and there are high authorities ranged on opposite sides. Now let it be remembered that we have been treating fever since the days of Hippocrates, that by an uniform system of observation we might, even in one year, have the symptoms recorded in about 100,000* cases of typhus fever in these countries; and let us ask ourselves, is this a state of things that ought to exist? The phenomena of nature are passing before us, and we refuse to note them as they are noted in other sciences, and we have received our reward. It is not our wish or intention to underrate any of the valuable additions that have been made to our knowledge of the treatment of many formidable diseases within the present century; but it surely must be considered a lamentable circumstance that there is scarce one of these improvements that are not disputed by men of the very first authority in the Profession. Thus with regard to the specific effect of mercury in arresting inflammation in the subacute or chronic form, it is now upwards of sixty years since not only this power was distinctly stated to exist with regard to subacute inflammation of the liver, but the generality of the principle, and its applicability to the same forms of inflammation in almost every organ of the body, was very forcibly insisted on by Dr. Robert Hamilton, of Lyme Regis. We will venture to say, that there is scarce a city in these kingdoms in which many most

* As in general only the worst cases of fever are brought to hospital, and the mortality in hospital is about 5 per cent. on the admissions, it may perhaps be safely stated that it is no more than 2½ per cent. on the whole of the cases occurring in and out of hospital, or about 1 death in 40 cases. As the deaths by fever in England and Wales, according to the Registrar-General's Reports, are about 18,000 annually, we have 40—18,000, or 72,000 for the number of cases of fever that occur annually among the population of England and Wales. If the population of Scotland and Ireland be taken in, we should probably have not less than 1,200,000 cases; and if only one-twelfth of these are admitted to Hospital, we should have annually about 100.000 cases under observation in these kingdoms; numbers which, if their symptoms are daily noted under a uniform system, would before many years, yield the richest results in prognosis and treatment.

respectable practitioners might not be found who would hold in ridicule the idea of its want of efficacy in such circumstances. It is stated in an excellent article on inflammation by Dr. A. Crawford and Dr. Tweedie, in the Cyclopædia of Practical Medicine, that "subsequent experience has amply confirmed the practical deductions of Dr. Hamilton as to the efficacy of calomel and opium in the treatment of inflammatory diseases; yet Dr. Alison says, that "in the opinion of many of the best informed members of the Profession there has been much exaggeration in all these statements,"* and talks of the remedy in every way so slightingly, that a young medical practitioner, who must always look to authorities as his guide, would certainly be disposed to place little reliance on its virtues. Again, with regard to the specific effect of tartar-emetic in large doses frequently repeated—this is so remarkable in many cases of acute rheumatism, that in those in which it is applicable no remedy at all appears to approach it in efficacy; and in our experience of it nothing could be more distinct than the fact, that if it produced nausea or diarrhœa, it in general did no good, while if this symptom did not attend its exhibition, the only other effect that was observable was a rapid removal of all the rheumatic action. We have had lately in the Limerick County Infirmary a young woman in acute rheumatism, who was admitted in excruciating agony of three weeks' duration, and who walked home at the end of a week from her admission, without the slightest trace of her complaint, though the only medicine she took during her stay was a grain of tartar emetic every hour, and the only symptom it produced was a removal of her sufferings; yet Dr. Alison* seems to think, that this medicine cannot produce any effect in inflammatory diseases, except by inducing nausea, though the disadvantage of a want of "tolerance" of the medicine has been particularly insisted on by nearly all its advocates.

Surely these questions do admit of a distinct solution; surely it would be possible, by trials upon large numbers, conducted under an uniform system, to determine the question definitively,

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* Cyclopaedia of Practical Medicine, vol. i. p. 97, article, History of Medicine.

* Cyclopaedia of Practical Medicine, vol i. p. 96, article, History of Medicine.

what epidemic constitution, or what combination of symptoms in fever wine is useful in ; whether mercury has or has not a specific action in any particular stage upon subacute or chronic inflammation existing in any particular organ ; or to pronounce upon the amount of good that tartar-emetic is capable of effecting in acute rheumatism and other inflammatory diseases, and whether or no its asserted specific action on them is aided or interfered with by the occurrence of diarrhœa, nausea, or vomiting? The questions we have raised are not one-hundreth part of those of a similar kind that might be asked respecting the influence of particular modes of treatment in particular diseases. To all of which the answers are equally unsatisfactory. Thus it is that the young physician is blown about by every wind of doctrine; authority contradicts authority ; and if the public but knew the hard card he has to play in his early career, and the vague and indefinite balancing of arguments for or against a particular mode of treatment, to which, in spite of himself, he is driven in several critical and dangerous circumstances, they would perhaps be more disposed to indulge than censure the apparent want of success that sometimes attends his efforts.

Besides the disadvantage of our finding many questions of as great importance as those we have alluded to unanswered, or without any definite answer, we continually meet with expressions in medical works so extremely uncertain and vague, that, in the consideration of any particular case, it would be impossible for the student or young practitioner to guess what evil tendency is most probable during its progress, or what complication he ought to be more strictly on his guard against. The terms, "very frequently," "very rarely," "generally," "not unfrequently," "sometimes," however useful and necessary in general descriptions, have this inconvenience that their meaning varies with the temperament of the person to whom they are addressed. They ought never, therefore, entirely take the place of phrases of measurement. The absurdity of such a practice would perhaps best be shown by supposing it to exist in other sciences, in astronomy, for instance. The theory of the lunar motions is one of the most intricate and complicated in all astronomy, and its reduction to the general principle of gravitation, in all its details, has given more trouble to mathematicians than almost any

other question in that science. Indeed, the laws of its principal irregularities were discovered by patient observation long before theory accounted for them, and her place at any moment can now be predicted within a few seconds of space. Now, what would be thought of the astronomer who should content himself with describing her motion in general terms, and say, "The motion of the moon is subject to great irregularities; *sometimes* in her course to the eastward, she passes very near the planet Venus, but *much more frequently*, a considerable distance from it; *occasionally, however*, she passes over it, and then the effect is very pretty. This phenomenon is called an occultation, and occurs *very often* with the fixed stars, but is *more rarely seen* among the more distant planets," &c., &c. This, which is a mere star-gazing description, is not farther below the true science of astronomy, than medical science, in its present condition, is below what it will certainly become when cultivated universally on proper principles.

It is extremely melancholy indeed, to consider the loose, hypothetical, and contradictory answers which we daily receive to many questions in medical science, to contrast them with those we should receive to analogous questions in the sciences which concern the movements and properties of mere inert matter, and to observe the precision, accuracy, and strictness by which the latter are usually distinguished. The most pointed difference between them, however, we should find to be this, that in the former, in almost all questions of past observation, the person questioned generally refers you to his memory for information, and if you are not satisfied with that, you have none better to get; the latter refers you to his table, with which you cannot help being satisfied, if you can trust their correctness, that is, if you can believe them to be facts, and recorded with accuracy.

In these observations, and in others previously offered, it will be seen that we have glanced at the methods of investigation that have given other sciences their great superiority over medicine, viz., the submitting to rigid enumeration all things that present themselves to us in the shape of facts, and the complete exclusion of mere opinion or estimate in all matters that admit of measurement, whether in magnitude or frequency, or any change that admits of strict definition in other words, the adoption of "the numerical method." But it will be at once objected, that the

comparison is not a fair one: that the difference is extreme between the variety, complexity, and uncertainty that characterize most of the phenomena of the natural sciences, and the simplicity and uniformity that have always exhibited themselves in the physical; that there is no end to the fluctuations that present themselves, in vital actions for example, whether in health or disease in different individuals placed apparently under the same circumstances, whether we consider the actual degree, progress, or termination, and that these things stand in very forcible contrast to the certainty with which, from the extreme simplicity of its laws, we are enabled to predict the changes, whether of place or form, that take place in mere inert matter. But it must be remembered, that animal bodies are machines of great perplexity, in which many processes of different kinds are going on together, under the influences of at least three great principles, the vital, mechanical, and chemical, which interfere with and mutually modify each other's action, and it will not be considered surprising that the results should exhibit great variety, nay, it would be very surprising if they did not, and it is by no means necessary to suppose a want of uniformity in the action of natural principles to account for such variety as they present to us, Were the solar system, instead of consisting of a certain number of bodies whose orbits do not deviate much from one plane, to consist of a much larger number, placed at every variety of distance, and with orbits inclined to each other at every possible angle,* it is not impossible to conceive the bodies of such a system moving each through its course under the the same principles as prevail in ours, yet, if they were at all closely set, what chance is there that any one of them would trace out a path conformable to any known curve? and when would the character of ellipticity in their orbits

* Sir John Herschell, on speculating on the dynamical condition of some of those extraordinary clusters of stars called "globular clusters," which are seen in the remotest regions of space, and in which the stars are so thickly set, and their number so great, that they cannot be counted by hundreds, supposes some conditions under which the individuals of a "globular cluster" might maintain themselves as a system, and describe each regular curve, under the same laws as prevail in ours, without being precipitated to the centre of the mass. Lardner's Cyclopaedia, Astronomy, page 415, note.

manifest itself under such a multitude of perturbations as they would be subject to? Notwithstanding which there can be no doubt, that the paths of such bodies would, in the circumstances we have supposed, amidst much confusion, present several remarkable analogies, and that by long and patient and persevering observation and accurate measurement, a uniformity of principle might be perceived in them, and the law of gravitation eventually deduced, though much more slowly than it was under the less complicated conditions that offered themselves to the attention of Sir Isaac Newton. Besides, in dwelling upon the varieties and uncertainties alluded to, the many striking proofs of a uniformity of principle in natural processes have been too much overlooked. We see facts every day that assure us that nature is acting under laws that do not alter, and the effects of which are only altered by circumstances. Look at the progress of cholera, for instance, and observe what a slight variety of symptoms was produced by so great a variety of circumstances as offered themselves, of climate, of race, and of constitution. Again, if a remedy is proved to have a particular effect on the frame in one country, or one age, it will be found to produce the same in every other, though we may occasionally find it difficult or impossible to produce it in some particular instances. We have no doubt that calomel would salivate in the hottest or the coldest country, or the earliest or the latest ages of the world; and when we meet an individual in whom we find it difficult to get up its specific action, we do not at once conclude that the general law is false, but that its tendency is baffled by some unseen cause, which careful observation may perhaps yet detect. This discussion might be extended, but enough has been said to show that in the natural as well as the physical sciences the laws that govern natural processes are general and constant. Indeed the contrary opinion would be equivalent to saying that like effects do not follow in like circumstances, the opposite of a truth which, as it has not been derived from any reasoning, so neither will any reasoning overthrow it. It is evident that our being shut out from a complete knowledge of all the circumstances of each case is the whole cause of our uncertainty in these sciences, and that the laws that regulate them are quite as general, and therefore, however complicated, admit of the same mode of investigation as in other sciences. It must

not be forgotten, too, that in physical science, until these modes of investigation were adopted, we could not have obtained the same proofs of uniformity we now possess. There was a time in chemistry when it might be fairly asked, if nature was uniform, or acting under uniform laws? Astronomy, perfect as it is now, has had its days of wild speculation and extravagant error; and with regard to the natural sciences, no one can doubt, that if we could only ascertain all the conditions of each case, with the exact number and force of the influences that guide any process to its end, we should be able to predict the event with as much certainty as we can now foretel the place of any of the heavenly bodies at a particular hour, or the chemical result of a particular mixture.

Such a certainty as this, however, is for obvious reasons not to be hoped for, and it is clear there can be even no approach to it, except by an accurate record and classification of all the phenomena of disease. It is vain to trust to one's memory for this purpose, and nearly as vain to trust to that irregular unmethodized custom of noting them, that has hitherto prevailed; a custom that makes it as easy now to begin to collect facts anew under a systematic and general arrangement, as to attempt to reduce to order the enormous, chaotic, and, in most respects, imperfect heap of materials that form the great bulk of medical records. The "numerical method" then, is the only one medicine can look to with any hope of attaining certainty as a science, and when we consider the many improvements that have been made within the last twenty years, the mode in which these have been effected, the growing disposition to hold hypothesis in distrust, and to rely rather on well ascertained facts, and, in particular, the exceedingly practical and valuable conclusions that have followed in various branches of it from even a limited application of this course, it cannot be doubted that the science of medicine is on the eve of a great and mighty revolution. For our own parts, we have been for years of opinion that the system of preserving the statistics of symptoms and treatment of disease under a uniform plan, will at no distant day supersede all others; and we have not the slightest hesitation in expressing our conviction, that this method opens a field for discovery in medicine, which would yield the richest fruits, and yet is al mos completely untouched. We are convinced that in the prog-

nostic of acute diseases, it offers applications of the theory of probabilities so important, that in after times people will be astonished at our neglect of them and that in treatment it will lead to conclusions so exceedingly definite and valuable, that when they come to be developed, there will certainly be no mincing of terms with regard to those respectable members of our Profession who are at present so unwise as to record their opinions against it.

Indeed the arguments on that side seem already in a great measure to have subsided; some excellent articles have appeared on its merits in different periodicals, and there is daily a strong and growing conviction in its favour. It might, therefore, seem unnecessary to take the subject up again, but we are induced to do so, partly because we apprehend that in some of these articles, however otherwise excellent,* there is rather a limited representation of its capabilities; and partly because we are of opinion, that, in the present state of the question, a few practical examples of its utility in medicine would have more weight with the medical Profession than the best theoretical reasoning.

It is one of the characters that attend the successful progress of any science, that its results take us by surprise; that they arise from facts which seem to bear but little relation to them, and in general are of a nature very distinct from any we have been led to anticipate. Hence, it will not be considered surprising if we should at present be unable to predict the exact nature or importance of the conclusions to which strict investigation in medicine may eventually lead us, and the argument with regard to such of its applications as we have no examples to offer upon, will be one of analogy. This is more especially the case with regard to the application of the theory of probabilities to the prognostic in individual cases of disease; a most important subject; one in which almost nothing has yet been done, and the careful prosecution of which would often, in doubtful cases, relieve the mind of the physician from much torment. We shall therefore offer some special examples of the result to which this theory sometimes leads in other sciences, to show that its application to medicine, whatever it leads to, will not be fruitless.

* See an admirable paper on this subject by William A. Guy, Esq, published in the second volume of the London Statistical Society.

We shall next give some instances in which the most important discoveries were made or missed in other sciences, by attention to, or neglect of, the "numerical method;" after which we shall give some remarkable instances of the important conclusions to which it is capable of leading in medicine itself; and, finally, we shall endeavour to remove some specious objections, which would not be of much importance, except for the respectable quarters they come from.

Speculations as to the probable duration of the life of a given individual may be divided naturally into two great classes; first, those in which a few personal circumstances only are considered in relation to the event; secondly, those in which it is necessary that all the circumstances should be so considered; the first refers to persons in a state of health, the second to those labouring under disease; the first form a subject for the prognostic of insurance companies which, in forming an opinion, general by assume, that all other circumstances are the same, except age; the second, form a subject for the prognostic of the physician, who, in forming his opinion, must take all the circumstances of the case into account, and compare them with his experience of the same circumstances in other persons, and with his memory of the event to which these circumstances led. Now suppose there are 1,000,000 persons of a given age, and that the question is, what is the probability that any given individual of this number will be dead at the end of thirty days? This probability would be given according to the usual rule, by a fraction having for its numerator the number out of the above that is found by experience to have died at the expiration of this period, and for its denominator the whole number observed upon; and the value of this fraction would determine the rate that any individual of them ought to be charged for the insurance of his life for thirty days. If we now suppose the whole of this number of persons to be attacked by fever, and the same question is asked, it is obvious that the numerator of the fraction will become much larger, because a greater number of persons in fever will die before thirty days than of persons in health. The fraction, however, can never become greater than unity, because the proportions that die out of a certain number in any specified time can never exceed that number; in other words, the numerator can never exceed the denominator, though it is possible it may equal it. Here too it is clear that the value of the fraction would

determine the rate that any one of these individuals ought to be charged by a company, supposing any such to exist, for the insurance of his life through fever, and this rate would of course be much higher than the other. But again, out of the above number of cases of fever a certain proportion would be found presenting through their whole course symptoms that were seldom or never found attended by a fatal result; and the fraction obtained from these cases would not differ much in value from that deduced from the observation of persons in health, while, on the other hand, we should find a certain portion of them presenting a combination of symptoms from which recoveries were very rare, and which therefore would yield a fraction differing little in value from unity or a total mortality; the simple circumstance then of a person being in fever is not sufficient to determine with closeness the probability of his death or recovery, and if the question was an insurance on his life through his illness, we might charge him a great deal too much or too little, according as he happened through the course of his disorder to be affected with mild or dangerous symptoms; the million of cases of fever would therefore have to be divided into several classes, each of which would be characterized by certain combinations of symptoms, and would yield a fraction indicating the degree of danger attending such combinations respectively. We should thus have a number of fractions varying in value, from the fraction of health to that which approximated to unity, or indicated the highest mortality; and if our classification was so minute, that the individuals of each class presented symptoms identical in number and degree, and that they were sufficiently numerous, these several fractions would be unchanging in value, and might be depended on. On calling to mind how very ignorant we are of the value of any one of these fractions—fractions the real value of which might be determined within very narrow limits, and that, too, by means of facts which are every day forcing themselves on our attention; symptoms which harass us by their dangerous associations and threatening aspect, and which, nevertheless, we obstinately refuse to record, except in the fleeting tablet of our memory; on comparing this ignorance with all that has been done to ascertain the amount of human mortality in a state of health, it is surprising to reflect how much will be undertaken for the sake of making money, and how little for the sake of preserving life. Yet it is obvious that if

the value of these several fractions was ascertained, the same principles that are every day applied in hygyenic medicine, might be applied in searching out the causes of a higher mortality among those symptoms and circumstances with which it was found associated; and this systematic attempt to remove or lessen them would in all probability be attended with as much success as the efforts in that science, and certainly with infinitely more than can ever be hoped for under the uncomparing, vague, and imperfect modes of investigation we at present adopt. It would be no small matter, too, as regards the personal comfort of the physician, that the danger of his patient would in most cases be expressed intelligibly in numbers, instead of having his mind continually on the rack, and perhaps often most harrassed in circumstances in which the danger was rather apparent than real.

These observations serve to point out some obvious applications of the theory of probabilites in medicine ; there are others, however, of a much higher order, for which it offers numerous applications, the nature of which can scarcely be conceived in the present low state of our notions regarding it. We shall therefore give some examples from other sciences, of the degree of probability that may be attained in circumstances in which certainty is impossible ; and though they are such as require a knowledge of the higher branches of mathematical analysis, and will only be properly appreciated in medicine when a better order of things has arisen, they must be considered of extreme importance, as their tendency is in many instances to investigate causes. The following passages are taken from a very interesting article on the Theory of Probabilities, in the Dublin Review for July, 1837.

"In every branch of inquiry which involves the actual use of our physical senses, the repetition of a process will always afford a series of discordances varying in amount with the method used, the skill of the observer, and the nature of the observation. If the observed discordances present anything like uniformity of character, we are naturally led to conclude, that they are not, properly speaking, the results of errors of observation, but of some unknown law, by which the predicted or expected result is modified. If the discrepancy merely arise from errors of observation, we must suppose that it will be sometimes of one kind, and sometimes of another ; sometimes producing a result larger than might have been expected, and sometimes smaller.

Now, having noticed a set of observations which do not agree, it is one of the first objects of the theory to settle what presumption should exist that the variations are accidental, (that is, totally unregulated by apparent or discoverable law) or that they follow a law which then becomes the object of investigation; the case taken by Laplace as an illustration will do for the same purpose here. It was suspected, that independently of local fluctuations, the barometer was always a little higher in the morning than the afternoon. To settle this point four hundred days were chosen, in which the barometer was remarkably steady, not varying four millimetres in any one day. This was done to avoid the large fluctuations which would have rendered the changes in question, if such there were, imperceptible. It was found that the sum of the heights of the barometer at nine in the morning, exceeded the sum of the heights at four in the afternoon, by four hundred millimetres; or one day with another by a millimetre a day. But what can we infer from such a circumstance is the first suggestion? A millimetre, or about the twenty-fifth part of an inch, is so very small a variation, that, considering the nature of the observation, and the imperfections of the instrument, it seems at first perfectly admissible, that mere instrumental error might have occasioned such a discrepancy. The theory of probabilities gives an entirely different notion: it appears, that it is many millions to one against such a phenomenon presenting itself, upon the supposition that it was produced by nothing but the casual imperfections of the instrument. A very great probability was therefore given to the supposition, that there really exists a diurnal variation of the barometer, in virtue of which *cæteris paribus* it is a little higher at one particular part of the day than at the other.

"In this way Laplace actually used the theory of probabilities as a method of discovery. He expressly affirms* that the irregularity in the lunar motions, which he afterwards showed to depend on the figure of the earth, was pointed out to him as not being of a merely casual character, by his having 'soumis son existence au calcul des probabilites.' Of another of his most brilliant results, he says as distincilty (p. 356), 'L'Analyse des Probabilites m'a conduit pareillement a la causes des grandes irregularites de Jupiter et de Saturne.'"

* Theorie Analytique des Probabilites, p. 355.

The second instance we shall give is nearly equally remarkable. It was an application by the same mathematician, Laplace, of the doctrine of chances, to show the probability of two comets being the same, from a near agreement of the elements, which are five in number, viz., the perihelion distance, the place of the perihelion, the place of the node, the inclination of the orbit, and the motion being direct or retrogade. It was assumed, that the number of different comets does not exceed one million ; a limit probably sufficiently extensive. The chance that two of these differing in their periodic times, agree in each of the five elements, within certain limits, may be computed, by which it was found to be as 1200 to 1, that the comets of 1607 and 1682 were not different. Halley had predicted with confidence its return in 1759 ; and as no one then had any notion that the probability of this was so high as we have just stated it, the question of its re-appearance was looked on with intense interest all over Europe, and all the influences to which it would be subject in its course, discussed by Clairaut, who had undertaken this most difficult and intricate piece of calculation. He found that the action of Saturn would retard its return by 100 days, and that of Jupiter by no less than 518, making in all 618 days, by which its expected return would be delayed. It was stated as the result, that its perihelion passage would take place about the middle of April, 1759, but that the limits of error might amount to a month at one side or the other. It actually happened on the 12th of March in that year.

It must be observed here, that in the theory of probabilities, the question as to the identity of these two comets resolved itself into the question, whether the discrepancy of the elements was owing to the comets not being the same, or to errors of observation. Here, therefore, as in the case last given, the application of the theory was to the investigation of causes, an application widely different from the ordinary one of averages. The intelligent practitioner will be at no loss to see numerous instances in medicine where the same theory might be applied with success to most important questions, if the facts necessary to the calculation were regularly collected under some uniform plan.

The discovery of the velocity of light, by the eclipses of Jupiter's satellites, is a very remarkable instance of a most brilliant discovery, and one totally unlooked for, arising out of the application of the numerical method to determine the times of their

future eclipses. Roemer, a Danish astronomer, apparently with this sole view had obtained the recorded eclipses of each satellite for many years, and having added up the observed intervals, obtained an average interval for each satellite, which he thought would give a tolerable approximation to the time of its future eclipses. Having done this he set about observing what agreement there was between these predicted times and observation, and was surprised to find considerable discrepancies. The following were the results:—Whenever the earth was at its average distance from Jupiter, the eclipses were observed to occur exactly at the predicted time; whenever the earth was at less than its average distance *they were seen sooner*, and whenever it was more than its average distance, *they were seen later*, but it was observed that they never anticipated or fell behind the predicted time by an interval of more than eight minutes; and, in point of fact, he found that by making an allowance for the variation of distance one way or other from the average distance, he could predict the times of the eclipses to a few seconds. The inference was obvious, that light took more time to go over the greater space, and less to go over the lesser, but the velocity assigned to it by these observations was so great (192,090 miles per second) that the truth of the observation was doubted by many, until the discovery of the aberration of light, by Bradley, from observations on the fixed stars, which brought a complete and most satisfactory confirmation of it. "The velocity of light," says Sir J. Herschel, "deduced from this last phenomeon, differs by less than one-eightieth of its amount from that calculated from the eclipses, and even this difference will, no doubt, be destroyed by nicer and more rigorously reduced observations."

The application of number to all those circumstances that admit of measurement by it, would indeed appear to be of the two rather more necessary in the natural sciences than in the physical. If there are many things in them about which we must per force rest in a state of uncertainty, it would appear only the more essential for us to apply a system of measurement at least to such points as admit of it, and not leave the whole to assumption or conjecture. A very striking instance of the extreme danger of this, even in the physical sciences, was shown in Sir Isaac Newton's Optical Researches. In this work, in which he made the most brilliant and beautiful discoveries, and during the prosecution of

which he exhibited an almost superhuman sagacity, he nevertheless committed an oversight which has astonished every body ; and this oversight arose from a neglect in almost this sole instance of the principle of admeasurement. He took it for granted, from his observations of the spectra produced by prisms of glass of different kinds, that the length of these spectra was always exactly proportional to the degree of refraction suffered by the mean ray. In other words, that the dispersion of light was always, and in all substances, proportional to the refraction of it. This conclusion has always been considered the more astonishing, inasmuch as the opposite one seemed, in almost every one of his experiments, to court his attention, and as it were force itself on him, and he had even a direct object in establishing it in preference, for if he could find any substance that gave a longer spectrum than another, when the refraction of the mean ray in both was the same, the improvement of the refracting telescope, with which view many of the experiments were instituted, would follow as a necessary consequence. Had Sir Isaac Newton subjected to accurate measurement the lengths of the coloured spaces in the spectra produced by different substances, and the lengths of the spectra themselves when the refraction of the mean ray was the same, he could not have been so deceived. As it was, after an infinity of labour, he came to the conclusion, that "the improvement of the refracting telescope was desperate ;" and left to his countryman, John Dolland, and to Mr. Hall, the glory of inventing, in a few years afterwards, the achromatic telescope, and, that too, by means of the very same steps which he had overlooked or neglected.

It would be easy to multiply such examples, but this paper is swelling to such a size that we must proceed.

That we may more clearly understand the advantage of the numerical method to medicine, let us consider for a moment in what it differs from individual experience, upon which physicians rely in their ordinary practice ; what objections apply to it, that do not equally apply to the latter, and whether it does not sometimes lead to influences of surpassing importance, which individual experience could never have suggested.

When in those diseases, the cause or essential nature of which is ill understood, a physician prescribes a particular medicine to allay any morbid symptom, or suppress any disordered action of the system, and finds that it has effected the desired object, he re-

sorts to a similar medicine for the cure of a similar disorder in another person, and notwithstanding his knowledge of the possibility of some idiosyncracy, or pecularity of habit, entertains anticipation of a favourable result, just proportioned to the amount of his previous experience.

When he has precsribed the same remedy under the same circumstances fifteen or twenty times with uniform success, he forms a confident conclusion in his mind as to its merit and applicability.

If, on the other hand, he has had apparent failure, or others in his neighbourhood have had failures, he is anxious to extend his trials and test the remedy by numbers. This, if correctly done, is the adoption of the numerical method in individual experience; it is, in truth, an imperfect mode of endeavouring to ascertain the universality of a fact.

He now finds by his extended personal experience, that the failures occur once in every eight or ten cases. His next effort is, to ascertain whether there be anything peculiar in the cases of failure, any symptom not observable in the others, and whether in these particular cases a different treatment may not answer; and when he has seized upon some symptom which he supposes to be peculiar; and adopted some new plan of treatment he awaits for months or years until he meets with a sufficient number of similar cases to test the truth of his supposition and his new plan of treatment. Here again is an imperfect adoption of the numerical method, an effort to arrive at a correct conclusion by a review of the practice of many years.

Excluding the few cases in the practice of medicine where the physician is fully cognizant of the immediate cause of disease, and thence infers an appropriate remedy, all his treatment of disease is founded on an imperfect recollection of past experience, or from some inductive reasoning which has from time to time gone on as a result of it. But as facts collected through a series of months or years, and consisting of an infinite variety of details, could be recalled by no memory, or their complicated relations embraced by no mind, we have, as a necessary result, endless differences and contradictions in the experience of individuals with regard to the same subjects. By the numerical method, the experience of the past is also recalled, but recalled correctly, because all the facts to which it relates have been classed in recording them. The number of all the individual facts can be counted,

their relative frequency compared in cases of a particular class, and their relative value determined by a comparison with facts of other classes which have also been reduced to similar elements. The physician who adopts the numerical system leaves no consideration out of view which could influence the mind of the physician who rejects it; he takes all the symptoms, favourable or otherwise, into consideration, he estimates them correctly, because he refers to his tables, which will retain the facts, rather than to his memory, which will not retain them; and when led to apply any treatment, arising out of inferences of some general law, of some universal fact, his application of such treatment is influenced and qualified by any known or supposed individual peculiarity in the case, every idiosyncracy of habit, any recognized condition of disease, as fully as if he had no statistical records to direct him.

To illustrate more clearly the value of the system we are advocating, let us for a moment contrast the unsatisfactory prognosis which, in ordinary practice, a physician forms, say of fever in a young woman on its fifteenth day, with that which is deduced by the statistician. The former draws some confused inferences or conjectures from his recollection of patients similarly circumstanced, and their results, but can arrive at no conclusion upon which he can rest with any confidence. The physician who has studied the statistics of such cases, even imperfectly as our present meagre records will permit him, on the other hand, can reduce has prognosis to some standard, the proportions of which can be closely estimated.

He sees that the patient is twenty-one years of age, and he finds the chances of recovery at that age are nearly twice as great as at forty-one.

He sees that the patient is a female, and finds that it gives one chance in three more of recovery than if a male.

That she has passed the fourteenth day, or second week, which is the fatal week of fever.

He can take a multitude of other circumstances into consideration, of the relation of which to death or recovery his tables furnish him with equally exact amounts—the presence of deafness the absence of subsultus tendinum, the moderate rate of the pulse at that period, the state of the sensorial functions; all these serve more or less to influence his conclusion, and to

give confidence to his anticipation and prediction to the result.

But the most striking and extraordinary illustrations of the influence of the numerical method on the practice of medicine, are to be found in these instances in which it has instantaneously, and by the tot of a sum, dissipated the universally received doctrine of an age in particular diseases—doctrines derived from the patient researches of a Hunter, and supported by the observation and experience of an Abernethy. It is needless to state, that mercury, up to a very recent period, was considered essential to the cure of syphilis, and those diseases, however otherwise undistinguishable, which got well without mercury were not deemed syphilitic. They were the pseudo-syphilitic diseases of Abernethy. This doctrine of syphilis and pseudo-syphilis was so specious that it threw an effective bar in the way of arriving at the truth by any inferences from individual experience. As soon, however, as an inquiry on the subject by the numerical method was instituted in the army, by Sir James Macgregor, a revolution was at once effected in the medical doctrines of the day. It was found, so far from mercury being essential to the cure of syphilis, that in 1940 cases treated without it, the cure of primary sores was effected in twenty-one days, on an average, in such as were unaccompanied by bubo, and in forty-five days, on an average, in those which were accompanied by it, while in 2827 cases treated by mercury, the average period of cure, when uncomplicated, extended to thirty-three days, and when complicated with bubo, to fifty days. It was ascertained, in fact, that every form of syphilitic sore may get well without mercury, and that primary syphilitic sores got well faster without it. These results would necessarily have entirely interdicted the use of mercury in the treatment of syphilis, had the inquiry gone no further, but in following it out it was ascertained that in the 1940 cures without mercury, secondary cases occurred in 96, while in the 2827 cases treated with mercury, only 51 had secondary symptoms. It hence appears that although primary syphilitic sores get well faster on the non-mercurial treatment, the security from secondary symptoms is less than when mercury is employed. The practical inferences from these facts are obvious.

1st. That it is better to employ mercury in the cure of primary syphilitic sores, although the cure go on more slowly, as it gives a greater protection from secondary symptoms.

2ndly. That as the disease may be cured without it altogether, there is no necessity and can be no object in carrying the use of mercury beyond slightly affecting the mouth.

3rdly. That for the same reason, where, from general delicacy of constitution or of any particular organ, the employment of mercury may be attended with hazard, it is not warrantable to use it at all.

The little approach that has been made to a scientific treatment of fever, or indeed of any other epidemic not originating in known local lesion, from the earliest age at which medicine became a study to the present hour, suggests irresistable reasons for believing our method of investigating its nature or testing the value of treatment is erroneous, and as little likely to lead to a true knowledge of the disease or its cure in future ages as it has proved to be in the past. The same may be said, even more forcibly of cholera, a frightful disease, which made a waste of human life in its progress round the globe sufficiently terrific to waken up all the energy of civilized man in the discovery or application of some effective remedy. It visited all nations, physicians had abundant practice, and the ingenuity of genius, and the boldness of speculation, and the daring of ignorance were exhausted in their efforts to devise a cure; yet what has been the result? absolutely that we have arrived at no determinate conclusions whatsoever regarding an appropriate treatment, if I except what little has been accomplished by the numerical method; and that the question regarding the merits of calomel, or opium, or brandy, or cold water, or blood-letting, or emetics, is now as wide of a satisfactory answer from the faculty in this country as when the disease first invaded us at Sunderland. From the varying nature of the disease, in fact, it was morally impossible that any individual member of the profession could arrive at correct inferences from his own practice merely, and it was equally impossible to draw correct inferences from the collected experience of many, the symptoms not having been recorded or the treatment pursued on any concerted plans. Let us, however, see what, under the same difficulties, even a very imperfect and limited adoption of the numerical method accomplished in the treatment of this disease, as far as relates to the merits of a remedy which came highly recommended to us from India; but at a very early period of the epidemic lost reputation in most places, and was finally almost discarded from practice.

During the prevalence of cholera in Limerick, one of the writers of this article was first attached to St Munchin's Hospital, where he tried every mode of treatment suggested by the Profession, with so little apparent success, that he lost all faith in the influence of medicine. In this frame of mind he was transferred to St. Micheal's Hospital, where the admissions were very numerous and the mortality alarming. He there found the calomel practice pursued with much more boldness and resolution, than any results he had witnessed could have induced him to adopt*. As it was little complicated with the use of opium or any other medicine which could materially interfere with its operation, he determined to become a mere observer of its results for one month, that he might obtain some numerical evidence of the good or evil which at least one powerful remedy was capable of effecting. To arrive at more accurate conclusions, he noted in each case, on admission, the presence or absence of the pulse at the wrist, and took care that the registrar or apothecary did the same whenever he was absent. He also, in conjunction with the other physicians, endeavoured to prevent as much as possible the admission of patients in the mere premonitory stage, which, however, was not always possible. At the termination of the month he found the gross amount of deaths was forty-seven, out of 165 cases, or less than one-third, which was not far away from the general mortality in most countries which had been visited by the disease. He had here a proof that the numerical method, if applied only to the admissions, general treatment, and gross results, could lead to no practical inferences; for the proportionate mortality in St. Michael's Hospital being nearly the same with the mortality in every hospital in the kingdom, he could deduce nothing either favourable or unfavourable to the treatment from any comparison. But his inquiry had a closer application, inasmuch as the patients were classified on admission into those not yet in collapse, or who had a perceptible pulse at the wrist, and those in collapse whose pulse was not perceptible. On casting up the tables and ascertaining the amount of mortality in these separate classes, he was perfectly astonished to find, that in the first class, affected with rice water vomiting, and purging, and suppression of urine, there was only five deaths

* From one to two scrupels were given every half hour until the symptoms gave way or death took place.

out of 119 cases, while there was forty-two deaths out of the forty-six cases admitted in a state of collapse. He had here at once evidence sufficient to lead to the most certain and confident conclusions. Cholera, so far from being a disease difficult of cure, was obviously more readily and certainly brought under the control of powerful remedies than any other known malady of so formidable a nature, and failures occurred entirely from neglecting to adopt a sufficiently bold and energetic treatment in the early stage; the only one in which the system is susceptible of the action of remedies. So long as the pulse was perceptible at the wrist, the calomel practice was clearly capable of arresting the disease in at least nine-tenths of the cases; but after the cessation of the pulse it did nothing, or it did mischief; the recoveries being perhaps less than might be effected if the cases had been wholly abandoned to nature until the period of reaction. These inferences were afterwards amply confirmed by the reports from other hospitals, in which the same notes of the condition of each case on admission were registered, and the same treatment adopted. The following are the reports referred to:

The Strand Hospital, from June 8th to June 22nd, 1832.

Admitted.		Died	Mortality.	Total Mortality.	Proportion admitted in Collapse, to total Admissions.
In the primary stage,	24	4	17 per cent.	48 per cent.	45 per cent.
In collapse, ...	20	17	85 per cent.		
	44	21			

St. Michael's Hospital, from June 14th to July 1st, 1832.

Admitted.		Died	Mortality.	Total Mortality.	Proportion admitted in Collapse to total Admissions.
In the primary stage,	74	12	16 per cent.	51 per cent.	52 per cent.
In collapse, . . .	80	67	84 per cent.		
	154	79			

The Nunnery Hospital, from June 8th to June 22nd, 1832.

Admitted.		Died	Mortality.	Total Mortality.	Proportion admitted in Collapse to total Admissions.
In the primary stage,	128	7	5½ per cent.	44 per cent.	54 per cent.
In collapse, . . .	154	117	76 per cent.		
	282	124			

St. John's Hospital, from June 8th to June 18th, 1832.

Admitted.		Died	Mortality.	Total Mortality.	Proportion admitted in Collapse to total Admissions.
In the primary stage,	419	29	7 per cent.	33 per cent.	39 per cent.
In collapse, . . .	264	185	74 per cent.		
	683	214			

St. John's Hospital, from its re-opening, August 21st, to its final Close, September 13th, 1832.

Admitted.		Died	Mortality.	Total Mortality.	Proportion admitted in Collapse to total Admissions.
In the primary stage,	59	8	13⅓ per cent.	42½ per cent.	51 per cent.
In collapse, . . .	61	43	70 per cent.		
	120	51			

Barrington's Hospital, from September 23rd, 1832, *to April 17th,* 1832.

Admitted.		Died	Mortality.	Total Mortality.	Proportion admitted in Collapse to total Admissions.
In the primary stage,	114	121	* *	55 per cent.	47½ per cent.
In collapse, . . .	103				
	217	121			

* The deaths of those admitted in the primary stage in this Hospital, and in the stage of collapse, were not distinguished.

Excluding Barrington's Hospital, in which the deaths of those admitted in the primary stage and in collapse were not distinguished, the following table gives a summary of the whole:

General Summary.

Admitted.		Died	Mortality.	Total Mortality.	Proportion admitted in Collapse to total Admissions.
In the primary stage,	704	60	8½ per cent.	39 per cent.	45 per cent.
In collapse, . . .	579	439	76 per cent.		
	1283	499			

The general correspondence of these reports is a striking proof of the truth of the inferences to which they lead, the mortality in the primary stages of cholera not having in any instance exceeded 17 per cent., and in the collapse being sometimes 85 per cent., and never having been less than 70 per cent. This latter very decreased amount of mortality was observed when calomel and all other powerful remedies were wholly suspended during collapse, as useless in that stage, and tending to mischief afterwards; when, in short, little was done for them beyond giving an occasional dose of mild cordial, and gratifying their thirst by abundance of cold water.

As this method of investigation was not applied to any other remedy but calomel, which happened at the time to be the popular practice, it could not be discovered, whether other influential remedies, as opium, tartar-emetic, ipecacuanha, &c., highly applauded by individual practitioners, bore any comparison with it in value. It hence became of greater importance to ascertain whether the salivation which calomel induced (in itself a very objectionable consequence) was necessary or could be avoided As most persons who were salivated recovered, it was very generally held that the salivation was the means of cure, and hence salivation became always the object. It is unnecessary to take up the reader's time with the inquiry which was instituted to determine this question, but simply to state, that it was found salivation was wholly unnecessary for the cure of the disease, and always resulted from exhibiting the calomel, after the true cholera

symptoms were obviously arrested ; no salivation occurring, however great the quantity of calomel given, if its use was suspended so soon as the vomiting, purging, and cramps, or the progression to collapse ceased. It was found, in fact, that patients did not recover because they were salivated, but they were salivated by the calomel already unnecessarily administered because they recovered and lived to be so.

These imperfect illustrations of the accurate conclusions to which the numerical method may lead in medicine, and above all in those diseases which have equally baffled the industry of the morbid anatomist, and the speculations of the most philosophic theorist, may suffice in some degree to show what might be accomplished by a perfect and well considered application of it in the investigation of disease. If it be true, as has been said, that epidemics, such as fever, or cholera, are continually varying in character from age to age, and from year to year, and that inferences drawn from the results of treatment in those which are past, can seldom or never strictly apply to new ones, it is obvious those variations of character must prove more perplexing to the physician, who is guided by the vague impressions of individual experience, than by him who can refer to the general laws, governing the course of past epidemics, and within an incredibly short period, test the connexion of those laws with the present ones, throughout every hospital in the kingdom. But this supposition of a constant change in the character of recurring epidemics, is to suppose those changes are without limit, which is hardly probable, and if not within limit, the same must come round again and be at once recognized by the medical statist. It may even be ascertained from the statistics of these epidemics in the progress of time, that they are subject to some general law, and recur at stated intervals, and in certain succession to other epidemics, and so pass away and come round again like the eclipses of the moon, the regular succession of which, in a particular order, was discovered by constant observation, many ages before they were known to be caused by the passage of the moon through the earth's shadow, and even before the earth itself was known to be spherical.

One of the most spacious objections to the adoption of the numerical method, and which it is essential to consider most fully,

is, that any inferences deduced from observations of the effects of remedies on the mass of the community, or from the average results of disease, varying more or less in every individual, can never apply correctly to individuals, any more than inferences regarding the value of life, or the wealth of an individual can be deduced from the average value of life, or average wealth of the community. This objection would strictly apply if the statistics in the numerical method were simply limited to ages and average results in the one case, or numbers and average wealth in the other. But they are not so limited, but include a record of every possible symptom, or circumstance in each case which could be of importance, or admit of tabular classification. Hence, in estimating the value of a particular life by the numerical method; not only would the average value of life at his age be considered, but the state of his pulse, respiration, his skin, his digestive organs, and even his temperament would be accurately estimated, and bear their satistical value in the conclusion arrived at. It might appear in such an estimate, that even the colour of the hair or eyes would determine us in assigning a shorter or longer period, or a lesser or greater value to the individual life. It is not meant to be asserted, that all this would give a close approximation to the value of an individual life, that is, the probable duration of life, not the average or commercial value; but it would be close in proportion to the minuteness, accuracy, and number of the facts or observations from which an inference is deduced, and beyond all measure nearer to the truth than any opinion founded on the strong impressions of mere experience. It is to be recollected, too, that the possession of correct statistical information relating to the nature, tendencies and treatment of any disease, does not at all preclude the physician from taking fully into consideration every peculiar or extraordinary circumstance or element not noticed in his tables, or exercising that tact which is so essential to the successful practice of the medical art.

Perhaps no circumstance has contributed so much to detract, from the merits of the numerical system, with English physicians, as the practical results of its adoption by its founder, M. Louis in his inquiries regarding the advantages of blood-letting in inflammation of the lungs. These inquiries would seem to lead to the absurd conclusion, that bleeding is injurious rather than

beneficial in such cases. But not to dwell on the probability that difference of age has a much greater influence on the course and results of disease than we at present attribute to it, it cannot be fairly assumed, as M. Louis has done, that the duration of a disease is any measure of its intensity; if the blood-letting diminishes the force and violence of a disease and leads to a successful issue, although it does not at once subdue it; perhaps all is accomplished which the nature of the malady will allow. Many complaints that we look upon as purely inflammatory, have, nevertheless, a specific character, and will progress under any treatment to a given period before we can note any appreciable decline; and it is, no doubt, by some such disposition in pneumonia we are to explain the fact observed by M. Louis, that in individuals bled late, or after the fourth day, the sputa lose their pathognomic character *two hours* after the first bleeding; while in those who are bled early, it never completely disappears before three days. After all, the number of cases from which M. Louis deduced his inferences, was not in any degree sufficient to warrant generalization, though quite enough to awaken our suspicions and induce us to inquire more closely, whether the influence of blood-letting is not at all events somewhat overrated in our present practice. While referring to this subject, it may be observed, that the errors of the statist are errors arising from a paucity of facts, and so long as nature is true to herself must be corrected by the acquisition of additional facts every day within his reach; while the errors of individual experience may never be corrected, as it may vary and even conflict in proportion as it extends. What can be more perplexing than the opposite experiences of the most celebrated men, apparently derived from opposite results, with regard to the comparative value of blood-letting and emertic-tartar in inflammation. They, in fact, left out all the qualifying circumstances which influenced the effects of treatment in the practice of each, and the result is, that the inexperienced physician, in his study of authorities, has to select between the experience of Rasori, Laennec, Peschier, Barry, Wolfe, Fontanelle, Téallier, Trousseau, Franc, Delpech, Lallemand, on the one side; and Strambio, Felix, Vacquié, Dance, Rostan, Andral, and Bouillaud, on the other!

It will after all be readily admitted, that those discoveries or improvements in medical treatment which arise from the discovery

of the causes or nature of disease are much more satisfactory, inasmuch as they are better understood, than any derived from general laws, founded on numerical calculations or comparisons. Thus, when a person complains of throbbing at the temples, ringing in the ears, appearances of motes or flashes of light before the eyes, headache, &c., if it be explained to us, that those symptoms depend upon a plethoric state, or upon increased vascular action in the vessels of the brain, we feel more satisfied in the advantage of depletion, than if we were told that in ninety-nine cases out of one hundred where such symptoms existed, depletion had been found successful in relieving them. In like manner, when Dr. Bateman tells us, that wine may be used in typhus fever, if the tongue be not parched, the skin be soft and moist, and the pulse open and fluent; and that it is inadmissible when the tongue is parched, the skin dry, and the pulse above 120, with the slightest perceptible sharpness in its beat, it is far less satisfactory to the practitioner, as being an inference from mere individual experience, than when he learns from Dr. Stokes, that excessive debility in fever usually arises from a softening of the fibres of the heart, indicated by the diminution or cessation of its impulse and sounds, and hence "that the diminution or cessation of impulse of the heart, the feebleness or extinction of the first sound, the preponderance of the second, or the proportionate diminution of both, are direct and nearly certain indications for the use of wine in fever."

But let it be recollected, that the truth of the supposed discovery of cause and effect in those very cases, and its value as a guide in practice, depends, after all, on inferences from numerical calculations; and so sensible is Professor Stokes of this fact, that notwithstanding the very extraordinary cases which he offers in support of his views, he states in a truly philosophical spirit, that "in the present state of the inquiry, he wishes it to be understood, his observations refer principally to the epidemics of the preceding year, and that further researches must be made to establish how far this may be applicable to typhus in general." There is no question in regard to the treatment of typhus fever of so much importance to be determined as the appropriate exhibition of stimulants. The practice has for ages been regulated by the prevailing doctrines, or prevailing opinions of the day, and stimulants

were given or interdicted as one plan or the other appeared successful in the individual practice of some leading practitioners. We have no hesitation in saying, that if general experience supports the conclusions to which Dr. Stokes has directed us, respecting the connexion between the physical signs of debility in fever, and softening of the muscular fibres of the heart, he will have accomplished not only the most important, but almost the only real improvement which has been effected in the treatment of that complaint in the history of medicine. But it must be observed, that there is nothing in the numerical method of investigation to hinder Dr. Stokes, or any physician of equal ability, from making this sagacious conjecture: on the contrary, it would, if properly carried out, be one of the most powerful instruments for the suggestion or detection of causes that could be thought of; for in the cases just specified, Dr. Stokes could, as a part of the system of marking every phenomenon deserving of notice, have noted the sounds of the heart with the other symptoms, and the appearances and condition of the organ after death. The only difference is, that a sagacious physician might be saved some labour by his tact in hitting off the causes of certain phenomena, while a man of more moderate skill would be only able to ascertain them by cautious induction from large numbers; besides, until Dr. Stokes's observations have been extensively tested in various epidemics, and his inferences established by numerical calculation, they must remain, like other ingenious medical doctrines, subject to much doubt, and it is possible may be found of very partial application. When correct, there are no doctrines so valuable as the anatomical or pathological doctrine of disease, because they lead to principles of treatment so simple and extensively applicable; but their correctness, however specious, the connexion of the external physical signs with the internal lesions, and the applicability of certain remedial measures, has to be tested by the numerical system before it can acquire any decided value. On the other hand, it will hardly be denied, that, independent of those doctrines and discoveries, if the numerical method had been resorted to in medical science in past ages, if the experience of the past had been systematically collected, so as to admit of deductions by numerical calculation, in the determination of the question at issue, we should be now in no doubt as to the evidences which indicate the use of wine in fever.

Dr. Stokes's own candid admissions bear us out in this assertion. No doctrines seemed more specious, or better supported by facts, than the anatomical doctrines of disease; those which referred all diseases to visible changes of organs, which taught, that inflammation was the first and principle morbid phenomenon, and that fevers were always the result of, or accompanied with, some local inflammation. Yet Dr. Stokes is of opinion that he could have saved many lives in his earlier practice, if he had trusted less to the doctrine of inflammation, and more to the lessons of experience, given us by men who observed and wrote before the times of Bichat and Hunter, that is, if he had depended on lessons deduced from the experience on numerical inferences of a few observant men, instead of being guided by doctrines founded apparently on anatomical or pathological facts.

It is not, after all, a matter for wonder, that the scientific physician, dazzled by the few brilliant discoveries of cause and effect in disease, and the almost immediate perfection of treatment as a result, should devote all his energies to the investigation of those causes in other diseases whose origin and nature are more obscure, perhaps inscrutable, in preference to a tedious search after general laws, bearing more accurately on masses of men than on individuals, and when most conclusive, and leading to the most successful practice, offering no explanation to the mind. If the motions of the planets, and their orbits, were as accurately known before the great law of gravitation was discovered as afterwards, their position in the heavens at any future time might be as correctly predicted, as it could now; but the capability of doing so would have been a mere result of experience, and not so gratifying as if deduced from the knowledge of a principle which applies to, and influences all the movements of nature, from the rolling of an apple to the wanderings of the comets. The error committed in these instances is not the application of the mind to the discovery of the causes or essential nature of disease, which in itself is a truly philosophical pursuit, but in doing so exclusively, and in doing so on the assumption of its general possibility. In no instance is a correct philosophical spirit so highly evinced, as in weighing the difficulties to be encountered before adopting a particular method of inquiry, and abandoning it for some more feasible one, when eventual success appears improbable. It is

not so much by what is most desirable as by what is most practicable, our efforts should be directed, and when the immediate connexion between cause and effect in disease seems too obscure to be reached either by the experiments of the physiologist, or the research of the pathologist, when, in fact, it has baffled the inquiring physician for ages, he should be content to direct his attention to the laws which diseased action observes, both under the influence of remedies, and when allowed to run its course uninterruptedly. These laws are always discoverable by the numerical method, when systematically adopted, and if the knowledge acquired is not all that could be desired, the inferences it leads to, are at all events as practical, and founded as strictly in truth, as if the intimate nature of disease was more fully understood.

THE END.

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APPENDIX.

No physiological inquiry, perhaps, ever so universally engaged the interest of the medical profession as that regarding the anatomical relations of sensation in living beings and the muscular movements which might or might not be attributed to it. Since the publication of the paper on sensation and consciousness (Prob. ix. p. 141), elaborate essays on the subject have appeared in almost every country in Europe, and many new anatomical and physiological facts have been discovered. The labours, however, of those who have most zealously directed their attention to it have, I think, rather tended to deepen the mystery, than to suggest any satisfactory explanation. They have raised new difficulties regarding every view heretofore entertained by physiologists, not excepting that which I have been endeavouring to sustain, and they have not, in any appreciable degree, removed the objections which have always applied to the system of reflex action as explained by Dr. Marshall Hall.

It is not meant for a moment to deny that a system of excito-motor nerves exists, that these nerves are independent of sensation, and that they evince their power after it is wholly extinguished. The experiments of M. Le Gallois demonstrated the fact very many years since; Abernethy recognized it; and Sir Charles Bell, as regarded a portion of that system—the respiratory nerves,—went some way in suggesting that beautiful explanation of it which Dr. M. Hall has since so successfully given. It is also far from my desire to detract from the great merit of that eminent physiologist, in pointing out and anatomically establishing that the excito-motor nerves were essentially distinct from the sentient and voluntary, and in connecting all the incident and reflex actions in the animal economy with a particular portion of the nervous structure, and with a great general law. I have objected to Dr. M. Hall's hypothesis, not because it is wholly without foundation, but because it assumes

too much, generalizes too much, and is far from explaining the great difficulty of the apparent existence of sensation after the removal of the brain, first suggested by the experiments of the French physiologists, and since accounted for by no one. No explanation—no theory on the subject can be considered in the least degree satisfactory until the boundaries of reflex, of sentient, and of voluntary action are more clearly defined; and least of all can it be considered so when the absence of sensation is necessarily assumed in cases in which all the ordinary (and as was heretofore imagined) the certain indications of sensation remain. In fact, Dr. M. Hall's investigations, however in part successful, seem to form but another step to a greater and more complete discovery, yet to be accomplished by the future physiologist.

I may again just shortly advert to some of the principal objections to Dr. M. Hall's theory, which have as yet been wholly untouched.

It appears to be ascertained, that a particular class of reflex actions can be very extensively and energetically excited in the frame, *after all the acknowledged evidences of sensation have disappeared.*

It appears, on the other hand, to be equally well ascertained, that a very complex class of movements, included by Dr. M. Hall in his excito-motory system, are always preceded by sensations in the perfect animal, are co-existent only with sensations, and never can be excited by any stimulus *after the ordinary signs of sensation become extinct.* By no possible excitement can sneezing, yawning, laughing, crying, be produced after sensibility is confessedly destroyed. It would be as irrational to connect these obviously distinct classes of movements with the same influence, as to include the peristaltic motion of the bowels and the motions of the muscles of the face, expressive of pain or emotion, under the same law.

The theory of combined muscular actions excited by impressions on the extremities of nerves, without the intervention of sensations, necessarily supposes definite and direct connections between the excitor and the reflex nerves in all cases. Yet Professor Alison has clearly shewn, "that no anatomical discovery—no conjectural connection of the nerves in their course or at their roots, can account for the muscular actions which arise in obedience to certain irritations applied; these actions accompanying or succeeding one

another in great variety, not according to the parts or nerves irritated, but according to the sensations excited."

Again, the system includes a rejection of the evidences of animal suffering, which have been acknowledged by mankind in all ages, and which are assumed instinctively by the brute. Spontaneity of action, the special adaptation of that action at the moment, in a way which we have no example of in the animal economy, except where such adaptation has arisen from an instinct, a sensation, or a rational volition, and the uttering of cries, are all set aside, as no longer offering evidence either of sensibility or suffering. When a pungent acid is applied to the nose of a brainless animal, it tries with its fore-feet to disembarrass itself of the cause of its pain, as if it were unmutilated. When a toe, or the sole of the foot is pinched, *it cries* and agitates itself, *and endeavours to withdraw and defend itself*, and yet we are required to believe that it has experienced no sensation! Is not the difficulty much less in such circumstances to suppose the animal felt for the moment, but having no organ of thought, memory, or self-consciousness, *the sensation was not recorded?* A momentary feeling, unrecorded or unretained by a memory, unrelated to the past or future, present for an instant and gone, would give one some idea of the nature of the sensation which might exist when the organ of thought and memory was taken away. When persons moan in their sleep without awakening, as they do frequently where slow organic disease is going on, and when they have no recollection whatsoever of having moaned on awakening them, it would not appear unreasonable to assume, they have experienced that kind of sensation or suffering which the brainless rabbit experiences when its whisker is plucked or its toe pinched. The subject, it must be admitted, is yet full of difficulties, and will require long and patient investigation before we can hope to arrive at any satisfactory solution of them.